Annual Subscription Price, $4. 108 Published Ten Times a Year.

ASTRONOMY and ASTRO-PHYSICS.

OCTOBER, 1892.

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Wm. Wesley & Son, 28 Essex St., Strand, London, are authorized to receive foreign subscriptions.

Entered at the Post Office at Northfield, Minn., for transmission through the mail at second class rates.

XUM

Astronomy and Astro-Physics.

NEW SERIES No. 8. OCTOBER, 1892. WHOLE No. 108

General Astronomy.

ON ASTRONOMICAL PHOTOGRAPHY WITH COMMERCIAL LENSES.*

WM. HARKNESS.

Since Barnard, Russell and Wolf have shown what excellent photographs of the heavens can be made with ordinary commercial lenses, persons anxious to attempt that line of work frequently make such inquiries as the following:

What sizes and makes of photographic lenses are suitable for stellar work?

What is the best ratio of aperture to focal distance?

Is it worth while to try anything of less than six inches aperture?

Can good work be done with a four-inch portrait lens, and is it probable that a lens of that kind can be obtained which will prove satisfactory without refiguring?

The proper answers to these questions all depend upon a few simple principles which may be stated briefly, as follows:

A.—The focal distance of the lens employed determines the scale of the resulting photographs, which are indeed maps of the heavens whereon each degree of declination is represented by a space equal to one-57th part of the equivalent focus of the generating lens; or more accurately:

One degree = 0.017452 (equivalert focal distance of lens).

For example, a lens of eight inches equivalent focus will give photographs on the same scale as Argelander's Uranometria Nova; one of ten inches focus will give photographs on the scale of Proctor's large star atlas; one of 44.5 inches focus will give photographs on the scale of Argelander's Durchmusterung star charts; and one of the 134.6 inches focus will give photographs on the scale of Chacornac's Atlas Ecliptique. In the latter work a minute of arc is represented by a space of one millimeter, and that is the scale which has been adopted for the great inter-

* Communicated by the author.

national photographic chart of the heavens. The focal distance of ordinary commercial lenses rarely exceeds three feet, and the maximum scale attainable with them is only about one-quarter that of the great international chart, or say 0.01 of an inch to a minute of arc.

B.—The ratio of aperture to focal distance affects the time of exposure, directly for nebulæ, and indirectly for stars. As all nebulæ have sensible angular magnitude, the time of exposure for them is given by the well known formula

$$t = C'\left(\frac{f}{a}\right)^2 \tag{1}$$

where t is the duration of the exposure, f the focal distance of the objective, a its clear aperture, and C' the exposure coefficient, which must be determined experimentally.

Notwithstanding the differences in their brightness, the stars themselves are all mere points without sensible diameter, but such is far from being the case with their photographic images. On account of imperfection in the lenses employed, atmospheric disturbances, spread of chemical action, and possibly other causes, the image of a first magnitude star sometimes attains a diameter as great as 0.18 of an inch by the time the image of a fourteenth magnitude star is just beginning to become visible. Hitherto no thoroughly satisfactory formula has been obtained for the time of exposure required by very faint stars, but as a rough approximation, sufficient for our present purpose, we may take

$$t = C''\frac{L^{(m-1)}}{a^2} \tag{2}$$

where L is the photographic light ratio, and m the magnitude on Argelander's scale. C'' must be deduced from the time of exposure of photographs of very faint stars made with lenses of known aperture, and in doing so one of the greatest obstacles is the difficulty of determining the true magnitude of the smallest stars shown. Insuperable systematic differences arise in the case of colored stars, but aside from them the best method of procedure is probably to make an experimental determination of the minimum time necessary for photographing the faintest white stars visible to the eye in an achromatic refracting telescope of known aperture. The visual magnitude of such stars can then be computed on Argelander's scale by means of the well known formula,

$$m = 9.2 + 5 \log a \tag{3}$$

where a is the clear aperture of the telescope, expressed in English inches.

As a guide to what should be aimed at in the photographs, it is desirable to know the magnitude of the smallest stars ever seen by human eyes, and for that purpose we have only to substitute for a the aperture of the largest telescope in existence—namely, that of the Lick Observatory, having an objective of 36 inches inches clear aperture—and thus we find $m = 16.98$, or say the seventeenth magnitude.

C.—Special care should be exercised in deciding upon the angular diameter of the field of view, because it limits the size of the photographic plates, and to a certain extent determines the amount of distortion which shall be tolerated.

In commercial photographic lenses the usual angular diameters of the fields of view, measured across the diagonals of the plates, are approximately as follows:

For wide angle lenses, 90°, or $2.0 f$.

For rapid rectilinear lenses, when used for taking views, 60°, or $1.1 f$.

For rapid rectilinear lenses, when used for taking portrait groups, 48°, or $0.9 f$.

For portrait lenses, 33°, or $0.6 f$,

where f is the equivalent focal distance of the lens.

When complete freedom from distortion is essential, the field must be very much more restricted. For their standard instruments of 13 inches aperture and 134.6 inches focal distance, the Paris International Conference adopted a square field measuring two degrees on each side. The celebrated optician, Mr. Alvan G. Clark, than whom there can be no better authority, told the present writer that according to his experience five degrees is the maximum possible diameter of field for photographing stars sharply with specially corrected portrait lenses. At the Cape of Good Hope, Dr. Gill uses plates six degrees square with a rapid rectilinear lens of about 60 inches focus and six inches clear aperture. When only pictorial effect is aimed at, somewhat larger fields are desirable, and with ordinary portrait lenses of about six inches aperture and 31 inches focus, Barnard, Russell, and Wolf use plates which give fields about fifteen degrees in diameter.

D.—It must never be forgotten that no matter how well commercial lenses may be corrected, and no matter how sharply they may define, they are ill adapted to any work requiring exact measurements because upon fairly large plates they give rise to

serious distortion which is usually symmetrically distributed about the centre of the field. A fine example of the careful determination of this distortion may be found in Sir G. B. Airy's Account of the British Observations of the Transit of Venus of December, 1874, Appendix V, pp. 14-19. As already pointed out, the only way of avoiding this distortion is by diminishing the angular diameter of the field, but if that expedient is adopted, other lenses will be found cheaper and more advantageous than those of the portrait or rapid rectilinear types.

As a guide to what may be expected from any given lens, it is desirable to have at least approximate values of the constants entering formulæ (1) and (2). An abstract of the data available for determining them is given in Tables I and II (pp. 646, 647).

The headings of the various columns in Tables I and II require no explanation beyond the statement that the letters f, a, t, m and C' have the same signification as in formulæ (1), (2) and (3). The values given for t and C' refer to fairly sensitive gelatine plates. For wet collodion they should be multiplied by six.

In Table I, each value of C' has been derived from the values of f, a and t given upon the same line with it; the formula employed being

$$C' = t\left(\frac{a}{f}\right)^2 \tag{4}$$

which follows directly from formula (1).

In view of the fact that the plates employed by different experimenters doubtless had various degrees of sensibility, and their optical apparatus various degrees of perfection, it is scarcely worth while to adopt any very refined method in deriving C'' and L from the data in Table II. By plotting the logarithms of ta^2 to the argument m, and drawing the best possible straight line through them, we find with sufficient accuracy

$$\begin{aligned} \log ta^2 &= 1.86 \text{ for } m = 9 \\ &\ 4.95 \text{ for } m = 15 \end{aligned}$$

And as formula (2) gives

$$\log ta^2 = \log C'' + (m - 1) \log L$$

we have

$$\begin{aligned} 1.86 &= \log C'' + 8 \log L \\ 4.95 &= \log C'' + 14 \log L \end{aligned}$$

whence, by solving the equations

$$\log ta^2 = 7.740 + 0.515\,(m - 1) \tag{5}$$

$$t = \frac{0.00550 \times 3.27^{(m-1)}}{a^2} \tag{6}$$

where t is expressed in mean solar minutes.

When the Paris international committee for the production of a photographic chart of the heavens began their labors, they naturally assumed L to be identical with Argelander's light ratio for visual star magnitudes, namely 2.512, and it is not easy to show why that assumption was erroneous, but the fact is now beyond question. In connection with our own result, it may be well to give some of the values found by other investigators.

In the "Réunion du Com. Int. Per., 1891, pp. 93-96," Dr. J. Scheiner gives

From plates exposed on the Pleiades

$$L = 2.5 \div 0.53 = 4.72$$

From plates exposed on artificial stars

$$L = 2.5 \div 0.71 = 3.53$$

From the diameters of stellar disks in the negatives of the Pleiades

$$\text{1st negative, } L = 2.5 \div 0.75 = 3.34$$
$$\text{2nd negative, } L = 2.5 \div 0.66 = 3.79$$

From a comparison of two plates of the region surrounding ε Orionis, one exposed one hour, the other eight hours,

$$L = 2.5 \div 0.63 = 3.97,$$

and from these data he concludes that L must lie between

$$2.5 \div 0.5 = 5.0 \text{ and } 2.5 \div 0.75 = 3.34.$$

In the *Monthly Notices* of the Royal Astronomical Society, 1892, Vol. 52, p. 265, Mr. R. L. J. Ellery gives, from experiments made at the Melbourne Observatory, $L = 3.16$.

Although these values of the photographic light ratio by Scheiner and Ellery are based upon a comparatively small range of star magnitudes, they acquire importance on account of the near agreement which they exhibit between results derived from so many radically different methods. The mean of all Scheiner's results, except the first, is 3.66, Ellery's result is 3.16, and our own is 3.27.

Reverting now to the questions propounded at the beginning of this article, and attempting to answer them in accordance with the principles developed, we are led to the following conclusions:

1. Thoroughly good photographs of the heavens may be taken with any commercial photographic lens giving sharp definition, and having an aperture not less than one-tenth of its focal distance, but the scale of the picture will depend upon the

TABLE I.—DATA RELATING TO PHOTOGRAPHIC TELESCOPES, AND EXPOSURES ON NEBULÆ AND STAR CLUSTERS.

Ref. No.	Experimenter.	Instrument.	f.	a.	Object.	t.	C′.
			Inches.	Inches.		Min.	Min.
1	E. E. Barnard	Willard (N. Y.) portrait lens	31.	5.9	Nebula in Andromedæ	258	9.35
2	H. C. Russell	Dallmeyer portrait lens	32.3	6.	Nebecula Minor	480	16.57
3	Max Wolf	Kranz euryscope	30.3	5.28	Milky Way about α Cygni	785	23.84
4	Max Wolf	Steinheil aplanatic	7.68	2.17	......	......	......
5	Isaac Roberts	Reflecting telescope	100.	20.	Various nebulæ	240	9.61
6	A. A. Common	Reflecting telescope	204.	36.	Great nebula in Orion	37	1.15
7	A. A. Common	Reflecting telescope	......	18.	......	......	......
8	Warren De la Rue	Reflecting telescope	120.	13.	......	......	......
9	L. M. Rutherfurd	Photographic refractor	153.6	13.0	......	......	......
10	Harvard Col. Observatory	Portrait lens, cor'd by Alvan G. Clark	45.12	8.27	......	......	......

Authorities for Table I; Arranged in Accordance with the Reference Numbers.

1. *Monthly Notices*, Royal Astronomical Society, London, 1890, vol. 50, p. 310. *Knowledge*, London, 1890, vol. 13, p. 174; *Ibid.* 1891, vol. 14, p. 232.

2. *Knowledge*, London, 1891, vol. 14, pp 50 and 112.

3 and 4. *Journal of the British Astronomical Society*, London, 1891, vol. 1, pp. 252 and 254. *Knowledge*, 1891, vol. 14, pp. 188, 231, and 232. The exposure for No. 3 occupied two nights.

5 *Knowledge*, 1889, vol. 12, pp. 108, 145, 148, 188 and 206.

6. *Monthly Notices*, Royal Astronomical Society, 1883, vol. 43, p. 255. *Publications of the Astronomical Society of the Pacific*, San Francisco, 1891, vol. 3, pp. 57 and 61.

TABLE II.—DATA RELATING TO PHOTOGRAPHIC TELESCOPES AND EXPOSURES ON STARS.

Ref. No.	Experimenter.	Instrument.	f.	a.	t.	m.	Log ta^2
			Inches.	Inches.	Min.	Magn.	
1	Lick Observatory	36-inch telescope with photographic corrector	570.	33.0	20.	13.2	4.3381
2	Lick Observatory	36-inch telescope with photographic corrector	570.	33.0	90.	15.	4.9913
3	Isaac Roberts	Reflecting telescope	100.	20.	205.	15.	4.9138
4	A. A. Common	Reflecting telescope	204.	36.	37.	14.8	4.6808
5	L. M. Rutherfurd	Photographic refractor	......	11¼	0.50	9.	1.8014
6	Henry Draper	Photographic refractor	......	11	137.	13.5	4.2195
7	Potsdam Observatory	Star Camera	135.	13.	0.40	9.5	1.8299
8	Oxford Observatory	Star Camera	135.	13.	0.92	9.	2.1917
9	Oxford Observatory	Star Camera	135.	13.	6.	11.	3.0060
10	Oxford Observatory	Star Camera	135.	13.	80	14.	4.1222
11	Sydney Observatory	Star Camera	135.	13.1	0.50	9.	1.9336
12	Sydney Observatory	Star Camera	135.	13.1	2.	11.	2.5356
13	Sydney Observatory	Star Camera	135.	13.1	30.	12.5	3.7117

Authorities for Table II; Arranged in Accordance with the Reference Numbers.

1. ***Publications of the** Astronomical Society of the Pacific*, 1891, Vol. 3, pp. 60 and 61.
2. *Ibid.*, p. 60.
3. *Ibid.*, pp. 57 and 60.
4. *Ibid.*, pp. 57 and 60.
5. ***American Journal of Science***, New Haven, 1865, Vol. 39, p. 308. With an exposure of 3 minutes Mr. Rutherfurd obtained photographs of 9th magnitude stars upon wet collodion plates. In Table II the 3 minutes have been reduced to 30 seconds, because fairly rapid gelatine plates are about six times more sensitive than wet collodion.
6. ***Washington Observations***, U. S. Naval Observatory, 1878, Appendix I, pp. 226–228.
7. ***Institut de France***. Acad. des Sci., Reunion du comité International Permanent pour l'exécution de la carte photographique du ciel a l'observatoire de Paris en 1891, pp. 93 and 96.

8, 9, 10. *Ibid.*, p. 72.

11, 12, 13. Preparations now being made in Sydney Observatory for the photographic chart of the heavens. By H. C. Russell. Read before the Royal Society of N. S. Wales, July 1, 1891, pp. 3 and 4.

focal distance of the lens. Nevertheless, if the original negatives are inconveniently small, it will usually be possible to make enlargements from them.

2. In order to get photographs of nebulæ showing as great an extent of nebulosity as those taken by Barnard, Russell and Wolf, it is only necessary to use the same exposure coefficients as they have done. That is, the times of exposure must be calculated from formula (1) with the values of C' given in Table I.

3. Formulæ (1), (5) and (6) show that the time of exposure for nebulæ is proportional to the square of the ratio of the focal distance of the lens to its aperture, and is independent of the size of the lens; while the time of exposure for a star depends only upon the aperture—that is, the size of the lens—and is independent of the ratio of focal distance to aperture. From this it follows that a small lens may photograph a nebula as rapidly as a large one, or even more rapidly; but in order to photograph faint stars, the lens must either be very large, or the exposures very long. The limiting magnitude reached on any negative can be calculated quite approximately by formula (5) or (6).

On account of the undue confidence in photography which is now so much the fashion, it seems desirable to point out that a photograph of a nebula may present a very different appearance from the object itself. To illustrate this, imagine two negatives of the same nebula, one exposed $4^h\ 22^m$ with Barnard's portrait lens of 5.9 inches aperture and 31 inches focus, and the other exposed $3^h\ 58^m$ with Robert's telescope of 20 inches aperture and 100 inches focus. According to formula (1) these two negatives will have the same exposure coefficient, and will be quite identical with respect to the amount of nebulosity shown, but according to formula (5), the first will show stars down to the 13.1 magnitude, while the second will show them down to the 15.1 magnitude; or in other words, the second negative will exhibit three or four times as many stars as the first. Of course the two pictures will present a very different appearance. Which is correct, and does either of them represent anything that can ever be seen by examining the heavens visually through a telescope? Furthermore, in view of such facts, have we any reason to be surprised at the differences which notoriously exist between the best drawings of nebulæ and photographs of the same objects?

WASHINGTON, D. C., August 13, 1892.

THE PLANET SATURN AND ITS SATELLITES.*

WILLIAM H. PICKERING.

The planet Saturn has proved to us of all bodies in the solar system perhaps the most disappointing. With superb definition, and almost unlimited magnification, it was hoped that its assumed analogy with Jupiter would permit us to observe something not visible under more adverse circumstances. Nothing has been found, however, that could not be seen with a six-inch telescope at home. The planet is of course a very beautiful object under one thousand diameters, every belt and shadow perfectly defined, and presenting, save for the loss of light, the same appearance that it would have with the naked eye from one of its own satellites, but nothing new is seen, no more detail is developed. The planet seems to be a mere dead mass of cloud,—and saving for a few faintly marked belts, to be as structureless, and uninteresting as the planet Venus. It gives no evidence of those rapid currents and violent outbursts, due presumably to intense internal heat, that render Jupiter such an interesting body when viewed through a large telescope. The surface seems perfectly quiescent like that of Uranus and Neptune, as if the planet had entered upon a phase of its existence where the central nucleus still gave out sufficient heat to retain gases such as steam in the form of cloud, but where violent upheavals can no longer occur, except under exceptional circumstances. The three outer planets it seems to me should be classed together, while Jupiter stands alone by itself.

A careful study of the surface has been made upon eighteen different nights since the first of February, in the hope of finding some spot or other detail similar to that recently described by Mr. Williams. Unfortunately nothing has been found, and we must consider him to have been unusually favored by fortune, and his observations to be the more important on account of their rarity. It is to be hoped that in the future, should distinct markings be discovered upon either Venus or Saturn, that the matter will be communicated at once to astronomers generally, by telegraph, in order to secure as many observations upon them as possible. Hazy and indistinct markings upon Venus are of course often seen, and it is possible that some of them are genuine, and not due to our own atmosphere. I have noticed how-

* Communicated by the author.

ever, that it is much easier to find such markings when the limb of the planet is wavering than when it is sharp and well defined. Spots upon Venus to be of any real interest to astronomers should be sufficiently well marked to admit of exact measurement and subsequent identification.

While no detail other than the belts has been found upon Saturn, a mere statement of that fact may, on account of our favorable opportunities for observation, be worthy of record. In regard to facts pertaining to the satellites, however, we have been more favored. Titan readily yields to a power of 700 diameters, and presents a sharply defined disc appreciably darker than that of the planet. On the evening of May 6, while making a careful scrutiny of the surface of Saturn, a small semi-circular projection was suddenly noticed upon the following limb just north of the ring. This was seen to grow in size, and it was soon recognized that we were watching a reappearance of Titan from behind the planet. In about three minutes from the first observation the emergence was complete, and a narrow black thread separated the two discs.

The eye-piece which was furnished with our micrometer gives a magnification of 350 diameters, but it was found that for the delicate work required of it here this power was quite inadequate and another eye-piece was made by remounting the lenses of one which had been fitted for use directly in the telescope. With this latter eye-piece all of our more recent measurements have been conducted. Our first measures of the diameter of Titan were made in the ordinary manner, by setting the micrometer threads at a distance apart of one second, and estimating the diameter of the satellite in terms of this distance. This gave a result of $0''.8$. Later it was found that it was better to use as our standard dimension the diameter of one of the threads itself. The thread was found to measure .010 mm. and to subtend $0.''4$. The thread could be illuminated until it was of about the same brightness as the satellite, and much more satisfactory comparisons could thus be made. By this means it was found that the diameter of Titan subtended about $0''.7$ and that it did not exceed $0''.8$, nor was it less than $0''.6$. These measurements were made in the early part of June; they would therefore correspond to a diameter of exactly 3,000 miles. Employing Stone's value of the mass, $\frac{1}{4600}$ that of its primary, this would give a density of .38 that of the Earth. Previous determinations of the diameter of Titan, given by Young and Chambers, are 3,500 and 3,300 miles respectively. I have not been able to find the original authorities, as our library here is somewhat limited.

Observations upon Iapetus give a diameter of 0.″4, corresponding to 1,700 miles. It is much more difficult to measure than Titan, and the result is accordingly liable to a greater uncertainty. Tethys, Dione and Rhea appeared smaller and brighter than Iapetus, which, when these observations were made, was in the eastern part of its orbit, two days before opposition. As is well known, Iapetus undergoes a considerable change of brilliancy in different portions of its orbit, being darkest on the eastern side. As the Arequipa telescope is of only 13 inches aperture, even a geometrical point like a star gives a disc 0″.3 in diameter, and no matter how perfect the atmosphere, or how great the magnification, no disc smaller than this can be measured with it from theoretical reasons. The diameters of the smaller satellites can therefore only be measured with an instrument of larger aperture giving smaller diffraction images.

There are, however, still two methods left to us for determining the approximate diameters of these satellites. One is to watch them during eclipse, and note the time that elapses from the instant when the satellite clearly begins to fade, as compared with the others, until it entirely disappears. This time, compared with the velocity of the satellite in its orbit, will give us a minimum value for its diameter. This method was actually employed by Professor Young a few years ago with regard to Rhea, although I have not his results at hand. The other method depends on the assumption that they all have the same albedo as Titan. Then, given their photometric magnitudes, their true diameters can be computed. From observations made here, we know that this method could be safely applied to the three inner satellites of Jupiter. If we apply it to those of Saturn, we shall get the following results:

Satellite.	Magnitude.	Apparent Diameter. ″	Diameter.	Young.	Chambers.
Mimas	12.8	0.15	600	600	1000
Enceladus	12.3	.18	800	800	?
Tethys	11.4	.28	1200	1100	500
Dione	11.5	.27	1100	1200	500
Rhea	10.8	.35	1600	1500	1200
Titan	9.4	.70	3000	3500	3300
Hyperion	13.7	.10	400	500	?
Iapetus (mass)	11.4	.28	1200	2000	1800

In the above table the photometric magnitudes given in the second column were determined by Professor E. C. Pickering, and will be found in the Harvard Annals, Vol. XI, p. 276. The third column gives their computed mean apparent diameters upon the above assumption, the fourth their corresponding di-

ameters in miles, and the fifth and sixth, their diameters in miles according to Young and Chambers. It is probable that the albedo of Iapetus is less than that of Titan, and that its diameter is therefore greater than that given in the table, as indicated by the direct measurements.

Attempts have been made here to see the shadows of the satellites cast upon the planet; but hitherto entirely without success. The most favorable satellite for this purpose is Titan, which casts a shadow whose umbra measures 0″.52 in diameter. Unfortunately no transits of this satellite have occurred at a time suitable for observation. The satellite casting the next largest shadow is Rhea, the diameter of its shadow being 0″.28. Its shadow was looked for most carefully on the nights of May 21, and June 8, at a time when it was central on the disc. Although the conditions were very favorable, and various powers of from 450 to 2190 were employed, the search was entirely without result. The shadows of Mimas and Dione have also been sought for without success. In the latter case I was aided by my assistant, Mr. A. E. Douglas, and various powers from 160 upwards were employed. Neither of us were able to even glimpse the shadow, although the night was unexceptionable. When we consider how difficult it is with a small telescope to see the dark space between two moderately faint stars, separated by 0″.28, even when we can select two stars of the most suitable brilliancy, it is perhaps not to be wondered at that with such a brilliant light as the disc of Saturn surrounding the dark space upon all sides, the shadow is a very difficult object to detect.

AREQUIPA, Peru, June 27, 1892.

SOME ADDITIONAL POINTS RELATING TO COMETS.*

GEORGE W. COAKLEY.†

In ASTRONOMY AND ASTRO-PHYSICS, No. 102, an attempt was made to demonstrate, that there was no repulsion by the Sun of any portion of a comet. Several mathematical friends of the writer, after careful examination of that paper, have testified their opinion of the solidity of the demonstration. In conducting the argument, the statement was made that a comet may best be regarded as a *mass of purely gaseous matter*. But the present

* Communicated by the author.

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prevailing view of a comet's constitution is that given by Professor Young in Art. 737 of his "Text-Book of General Astronomy, for Colleges and Scientific Schools." Professor Young states it as follows:

"Perhaps on the whole the most probable hypothesis is the one which has been hinted at repeatedly, that a comet is, as Professor Newton expresses it, nothing but a 'sand-bank'; *i. e.*, a swarm of solid particles of unknown size and widely separated, (say pin-heads several hundred feet apart), each particle carrying with it an envelope of gas, largely hydrocarbon, in which gas light is produced either by electric discharges between the particles, or by some other light-evolving action due to the Sun's influence. This hypothesis derives its chief probability from the modern discovery of the close relationship between meteors and comets."

The writer proposes, after disposing of another question, to discuss this theory of comets, proposed by Professor Newton of Yale College.

Admitting, for the present, Professor Newton's theory, as stated by Professor Young, how does it affect the demonstration, in No. 102, that there can be no repulsion by the Sun of any portion of a comet?

The particles of this "sand-bank," whether "pin-heads several hundred feet apart," or larger pieces of "gravel," or great boulders weighing many pounds, or even tons (all of which are required by the supposed "close relationship" between meteors, or rather meteoric stones, and comets), must form first the nucleus and head of the comet, which are always *attracted by the Sun*. Secondly, these particles must also form the train or tail of the comet, stretching away behind the head during its approach to perihelion.

If these "pin-heads or finer or coarser gravel, or large stones and metalic masses, forming the train are *repelled* by the Sun with a force greater than their *attraction* by the comet's head and nucleus, then they must be *retarded* in their approach to perihelion, while the head and nucleus are *accelerated* by the Sun's attraction. They must therefore separate from the comet's head, and move in a curve *convex to the Sun*, because of his *repulsion*, while the head and nucleus must move in a curve concave to the Sun because of his *attraction*.

Indeed, just as in No. 102, it was proved that if any of the comet's *gaseous matter* were subject to the Sun's repulsion *before perihelion passage*, it must be entirely *sifted out*, so that all of

the comet that passed the perihelion was necessarily subject only to his *attraction*, so the same thing must be true of the "pin-heads," or other solid matter. Indeed it ought to be evident that the demonstration in No. 102 is quite independent of the nature and distribution of the matter composing a comet, provided it be of a sufficiently loose and rare nature. The more *solid* the particles of which the comet is composed, the more closely they ought to be aggregated by their mutual attractions, and tend to form a *single solid body*, like the planets, provided the whole number of these particles forms a sufficient *mass*, somewhat comparable to the masses of the planets, though far less.

It seems worth while to consider whether we have not some clearer evidence of the limits of a comet's mass than is generally supposed. La Place has assigned a *superior limit*, at least of Lexell's comet, which passed very close to the Earth, and to Jupiter and his satellites, without appreciably disturbing the motions of either of these bodies. He assigns this comet a mass *not exceeding* $\frac{1}{5000}$ of the Earth's mass. Many astronomers have reduced this limit to almost nothing. Professor Young, whose work is quoted as one of the most recent, full and accurate on general astronomy, says: "Some have gone so far as to say that a comet, properly packed, could be carried about in a hat-box or a man's pocket, which, of course, is an extravagant assertion. The probability is that the total amount of matter in a comet of any size, though very small compared with its bulk, is yet to be estimated as *many millions of tons*. The Earth's mass is expressed in tons by six with twenty-one ciphers following (6000 millions of millions of millions of tons). A body, therefore, weighing only one-millionth as much as the Earth, would contain 6000 millions of millions of tons." This last is just about the weight or mass of the Earth's atmosphere. But in the edition of Professor Young's work from which I quote, that of 1888, he goes on to say, "The atmosphere of the Earth alone constitutes about $\frac{1}{250000}$ of the Earth's mass, and contains more than twenty-four millions of millions of tons." Here are two mistakes, oversights no doubt, and probably corrected in future editions which I have not seen. The mass of the Earth's atmosphere is stated about four times too large, and the *consequent* value in tons about one thousand times too small. La Place's limit of a comet's mass makes it about two hundred times the mass of our atmosphere. The writer has verified approximately Professor Young's statement of the Earth's mass in tons, considering it as a sphere with its mean radius.

The clouds that float in our atmosphere have no definite form; they have not sufficient mass to gather themselves up into any definite shape by their own attraction, whether in a globular mass, or any regular geometrical shape. Yet they are capable of pouring down upon the Earth many hundreds of tons of water. But when a comet is first seen at its greatest visible distance in a telescope, it usually presents the *circular disc of a spherical body*. In the case of Donati's comet, when first discovered, it was more than 180,000 miles in diameter. Yet the attraction of its mass must have reached out more than 90,000 miles to cause the particles at its surface to arrange themselves spherically about it. Even after its train was formed, the regular geometric figure of the greater portion of the train shows that the attraction at the nucleus was maintaining a certain *equilibrium* with the tidal action of the Sun in the direction away from him. The action of this mass then extended, with a prevailing force, to some millions of miles from the nucleus, or centre of gravity.

Professor Benjamin Peirce, of Harvard College, was, therefore, more nearly right in his opinion than Professor Young seems to think, with regard to the amount of a comet's mass. Professor Young says (Art. 719), "The late Professor Peirce based his estimate of a comet's mass upon the extent of the nebulous envelope which it carries with it, assuming (what may be doubted, however), that this envelope is gaseous, and is held in *equilibrium* by the attraction of solid matter in and near the nucleus; and on this assumption he came to the conclusion that the matter in and near the nucleus of an average comet must be equivalent in mass to an iron ball as much as 100 miles in diameter. This would be about $\frac{1}{300000}$ of the Earth's mass." This ratio of the iron ball's mass to that of the Earth has also been verified by the writer.

Professor Young says farther, "While this estimate is *not intrinsically improbable*, it cannot, however, be relied upon. We simply do not know anything about a comet's mass, except that it is exceedingly small as compared with that of the Earth."

To this the writer has to say that the *equilibrium* by the attraction of the comet's *mass*, at and near its centre of gravity, the nucleus, as stated by Professor Peirce (without the necessity of any *solid body* there placed), is *beyond all doubt;* and that, by means of this *equilibrium*, we do know, from Professor Peirce's calculation, that the *average mass* of a comet is *very probably* the $\frac{1}{300000}$ of the Earth's mass, or about 20,000 millions of millions of tons. This is a little more than three times the mass of our atmosphere.

The mass of the Earth is sufficient to control the motion of its moon in a relative orbit about its centre, at the distance of nearly a quarter of a million of miles. Also the Earth's gravity extends many millions of miles to disturb the motions of Mercury, Venus and Mars. It even has some slight effect on the more distant planets. Hence it is certain that a mass one-millionth part of the Earth's mass, like that of our atmosphere, would likewise, as a separate mass, extend its attraction to the distance of millions of miles, though in a proportionally less degree.

Suppose, therefore, that a mass of gas like our atmosphere, containing the same weight of 6000 millions of millions of tons, were placed as far as Jupiter is from the Sun, but moving in a plane highly inclined to the ecliptic, so that it should be far away from all planets of the solar system. Would it not at first gather itself up into a spherical form by virtue of the general attraction of its mass? Owing to the elasticity of the gas, its size would, of course, be far greater than the volume of our atmosphere, which is condensed by the Earth's attraction, one million times as great as that of the gas. We should expect it to expand to a diameter of perhaps 200,000 miles, its mean density being very small, but having a greater density at the centre of the sphere, or at its centre of gravity.

If this atmospheric mass were describing a nearly circular orbit about the Sun, it is quite certain, from the formulæ investigated in ASTRONOMY AND ASTRO-PHYSICS, No. 103, that the Sun would exert a tidal force upon it, not only drawing it out into an ellipsoidal figure, with the major axis directed towards the Sun, but also transferring the centre of gravity of the atmospheric mass towards the Sun. This centre of gravity of the atmospheric mass, during its revolution, would be always directed towards the Sun. The ellipsoidal mass would have a nearly constant *figure of equilibrium*, depending upon the contest between the attraction of its own mass and the tidal disturbing force of the Sun, at that nearly constant distance. The atmospheric mass would also necessarily rotate around an axis through its centre of gravity, perpendicular to the plane of its orbit. It would also constantly turn the same face towards the Sun, for the same reason that our moon turns the same face towards the Earth.

Since one of the foci of the ellipsoid, and the centre of gravity of the mass, have both been displaced *towards the Sun*, we may suppose the centre of gravity always to coincide with this focus, as was the case when the figure was spherical.

If, now, we change our supposition as to the form of the orbit described about the Sun, and suppose it to be an ellipse of great eccentricity, and that the atmospheric mass is approaching the perihelion of its orbit, it is evident that, on account of the diminishing distance from the Sun, his tidal disturbing force is increased, and that the previous *equilibrium*, between this force and the attraction of the atmospheric mass, can no longer exist. A new equilibrium must be established by a farther change of the ellipsoidal figure, which must become more eccentric, the focus nearest the Sun, and the centre of gravity of the atmospheric mass, retreating together towards the Sun more than they had previously done. With each change of distance from the Sun, the *figure of equilibrium* for the atmospheric mass would have to be constantly renewed. It was undoubtedly the investigation of such figures of equilibrium, actually observed with regard to several comets, that enabled Professor Peirce to determine the mass of an average comet. There can be *no doubt at all* of its *probable accuracy*. Indeed, would not the atmospheric mass, which we have been supposing to revolve around the Sun in a very eccentric orbit, present all the phenomena of a comet?

Among the phenomena of comets, noticed by Professor Young, is the diminution of the comet's *head* as it approaches perihelion, and its increase in passing away from this point. It has been shown in No. 103, ASTRONOMY AND ASTRO-PHYSICS, that the radius of the *spherical head* is the distance from the nucleus, or centre of gravity, to the surface of the comet *nearest to the Sun;* and that this distance is constantly *decreasing* as the comet approaches the Sun, and necessarily *increases* on its retreating from perihelion. So that the cause of this phenomenon is fully explained by the Tidal Theory of the Forms of Comets.

The cause of multiple tails of some large comets may perhaps be found in the necessary change of eccentricity of the comet's *figure of equilibrium* as it approaches perihelion, or retreats from it. It is evident, on this theory, that when the tail is first formed the eccentricity must have increased from less than unity, while the figure was *ellipsoidal,* to the full value of unity, for a *paraboloidal figure*. But, as the comet approaches the Sun still nearer, the focus and centre of gravity must still retreat towards the Sun. So that the eccentricity will be greater than unity, or the *figure of equilibrium* will become an *hyperboloid*. Moreover, the interior strata of the comet's figure may, both in the case of the parabolic form and the hyperbolic, have each its own eccen-

tricity, or separate centre of gravity. The nucleus must therefore be divided, as it is sometimes seen to be, especially in the great comet of 1882. These separate foci of the several strata of the comet, one within the other, would produce different divergencies of the hyperbolic branches especially, and thus account for the differently diverging tails of a great comet.

In Art. 727 of Professor Young's work, he gives a fine cut of the "head of Donati's comet, Oct. 5, 1858, (Bond)," showing the way in which, at that date, about five days after perihelion-passage, it threw off "jets and streamers of light," "more or less symmetrical envelopes," which followed each other at intervals of some hours.

These phenomena may perhaps be explained by the fact that, as the head of the comet grew smaller, on approaching perihelion, with the *same mass* at its general centre of gravity, the pressure from the *greater attraction* towards the centre of the head, due to the diminished distance from this centre of gravity, would be greatly increased, and the elastic strata of gas would have to yield to this pressure. On retreating from perihelion, the head increases its radius, and the attraction towards the centre of gravity, and consequent pressure on the elastic strata within, *must diminish*. These compressed strata of elastic gas therefore naturally *push their way outwards* towards the surface of the comet's head, where the relief of pressure had taken place. On retreating farther from the Sun, other such remittances of pressure occur, and new envelopes rise symmetrically towards the Sun. Thus these phenomena seem to derive an easy explanation from the Tidal Theory of the Forms of Comets and have received no explanation from any other source.

Let us now consider the constitution of comets consisting of "pin-heads several hundred feet apart, each particle carrying with it an envelope of gas, largely hydrocarbon." Or, as Professor Young elsewhere expresses it, "the head of a comet is a swarm of meteoric stones; though whether these stones are many feet in diameter, or only a few inches, or only a few thousandths of an inch, like particles of dust, no one can say. In fact it now seems quite likely that the greatest portion of a comet's mass is made up of such particles of solid matter, carrying with them a certain quantity of enveloping gas."

How much "gas, largely hydrocarbon," or otherwise, could be condensed upon one of these "pin-heads," or even upon one of the meteoric stones, "many feet in diameter," unless the many feet were many millions of feet? A meteoric stone, one hundred feet

in diameter, would be incapable of confining by its gravitating power, its mass, any considerable amount of gas; and the pin-heads hardly any at all. If the great mass of the comet itself were chiefly gaseous, then a comparatively small number of these stony masses might be imbedded in it.

But what would be their position in the cometary mass? Clearly, on account of their superior density, at the very center of gravity where the nucleus is. Not only so, but the attraction of the comet's great mass, nearly 20,000 millions of millions of tons, according to Professor Peirce's calculation, would forbid their remaining several hundred feet apart from each other, and would aggregate them all, whether "pin-heads" or larger, into one *compact solid mass* at the nucleus or center of gravity. It would be quite impossible for any *loose, scattered solids*, large or small, distant from each other several hundred feet, to constitute a mass any way comparable to that computed by the late Professor Benjamin Peirce, one of our first mathematicians and astronomers. It would require an enormous repulsive force, such as we see nowhere in nature, except in the elastic force of a gas, to prevent these solids from rushing down towards, and adhering as closely as possible to, the comet's center of gravity. But the elastic force of the gaseous comet would not prevent this rushing inwards of the *solids*, on account of their greater density.

These considerations are probably sufficient to justify the rejection, in all its forms, of Professor Newton's "sand-bank," or "gravel-bank," or "meteoric stone" theory of a comet. But, says Professor Young, "this hypothesis derives its chief probability from the modern discovery of the close relationship between meteors and comets." The close relationship between comets and meteors, that is, shooting stars, so-called, and meteoric swarms, like those of the Leonids, the Perseids, the Andromedes, and other similar meteoric showers, is freely admitted as conclusively proved. But what evidence is there that the phenomena of the *fall of meteor stones* or *meteorites*, as distinguished from *meteors*, has any such relationship with comets?

Professor Ball, the Astronomer Royal of Ireland, has closely considered this question. He shows that while the meteors, or shooting stars, have generally a very swift motion, passing out of sight in from half a second of time to nearly one second, the meteorites, from which the stony masses are derived, frequently remain in view for a minute or more, having thus a velocity about one sixtieth of those of cometary meteors, if at the same distance from us. He points out other differences; but the most

important is that, during even the greatest shower of meteors, like the great November showers of the Leonids, while the meteors seem to fall by the million, and for hours, like flakes in a snow-storm, yet nothing solid has been found to fall to the Earth's surface at such times; "with one exception," says Professor Young, in the case of the Mazapil meteorite. The account of this by Professor Young is as follows:

"As has been said, during these showers" (of meteors proper), "no sound is heard, no sensible heat perceived, nor do any masses reach the ground, with one exception, however, that on Nov. 27, 1886, a piece of meteoric iron, mentioned in the list given in Article 758, fell at Mazapil in northern Mexico during the shower of Andromedes which occurred that evening. Whether the coincidence is accidental or not, it is interesting. Many high authorities speak confidently of this particular iron meteor as being really a piece of Biela's comet itself."

But we may reasonably ask, on what grounds do these "high authorities" speak so confidently? Professor Newton tells us that meteoric stones fall nearly or quite every day on some part of the Earth. Hence it would not be unreasonable to expect one at Mazapil, or elsewhere, on Nov. 27, 1886, whether the Andromedes were then bombarding our atmosphere or not. But why should only one of the great number of Andromede meteors of 1886 reach the Earth, and no other of these, nor any of the millions from the Leonids, the Perseids, and other swarms be found to reach the Earth? Clearly the Mazapil *meteorite* has only an accidental coincidence with the Andromede *meteors*.

The perfect transparency of a comet, through many thousands, or millions of miles of its volume, tells us that it is a very rare gas. In Article 720, Professor Young states, "This estimation of the density of a comet is borne out by the fact that small stars can be seen through the *head* of a comet 100,000 miles in diameter, and even very near its nucleus, with hardly any preceptible diminution of their lustre." Other astronomers have frequently observed the passage of the *central portion* of the comet's head at or near the nucleus, over a small star without any sensible loss of the star's light. From such facts we can only infer that even the densest part of a comet is a very rare, transparent gas.

Observations of comets with the polariscope prove that nearly all their light is the Sun's light *reflected* by a rare gas. Observations with the spectroscope seem to show that a *small portion* of their light, when sufficiently near the Sun, is somehow excited within the comet itself, but that the substance of the comet is largely, if not "wholly gaseous, chiefly hydrocarbon."

If the comets were *self-luminous*, or largely so with only the addition of some sunlight, then they should be easily visible at all distances. They could not escape the reach of the telescope at their aphelions. But instead of this being the case, they are only visible when comparatively near the Sun and the Earth. Their brightness at various distances from us and from the Sun follows the same law of increase or diminution as that of the non-luminous planets. Professor Young mentions some slight exceptions to this law in the brightness of certain comets; but perhaps this may be sufficiently accounted for by the irregular transparency of our own atmosphere. All astronomers know that there are times when no clouds are apparent, and yet there is no *good seeing* with the telescope, on account of the condition of the upper strata of our atmosphere. On the other hand, even when there is a light mist, there may be *good seeing* and *sharp definition* in the telescope.

From the ascertained mass of an average comet, and from its transparency, the "sand-bank" theory of the constitution of comets, proposed by Professor Newton of Yale, must be rejected. There is no close relationship between comets and meteorites, or meteoric stones. These latter can be explained, with great probability, as having a quite different origin from the meteoric ring-systems which produce the swarms of gaseous meteors, the Leonids and others. In rejecting Professor Newton's theory of comets, I should regret saying anything that could detract from the great merit of his investigations relating to the great November showers of meteors, the Leonids, which investigations prepared the way for computing the orbit of this meteoric ring, and thence of its connection with a comet pursuing the same orbit.

THE DOUBLE STAR, *OΣ* 224.*

S. W. BURNHAM

This pair has been under observation since 1843, and there is no doubt now concerning the physical relation of the components. The period is evidently a long one, since the angular motion is only about 60° in the time covered by the measures. It is always close enough to be difficult with most of the instruments used in measuring it, and therefore some of the observations have large errors in the position-angles. The total change, how-

* Communicated by the author.

ever, is sufficient to obtain the approximate elements, and for this purpose I have collected all the measures, and give them below in chronological order:

	°	"		
1843.22	13.7	0.35	Madler	2*n*
1844.31	20±	—	O. Struve	2*n*
1845.30	13.6	0.20	Madler	1*n*
1851.27	352.6	0.48	O. Struve	1*n*
1851.28	17.5	0.25	Madler	1*n*
1857.34	3.6	—	Secchi	1*n*
1861.26	348.8	0.59	O. Struve	1*n*
1868.03	339.2	0.5±	Dembowski	4*n*
1871.31	328.4	0.59	O. Struve	1*n*
1872.31	336.8	0.55	O. Struve	1*n*
1873.23	329.8	—	Dembowski	4*n*
1879.32	315.7	0.35	Shiaparelli	4*n*
1880.16	334.3	0.62	Burnham	1*n*
1881.25	316.6	—	Doberck	2*n*
1882.27	309.9	—	Doberck	1*n*
1883.71	330.2	0.53	Engelmann	7*n*
1884.21	326.0	0.55	Perrotin	4*n*
1887.27	315.6	0.52	Schiaparelli	4*n*
1892.37	313.6	0.48	Burnham	4*n*

The last set of measures were made by me with the 36-inch refractor before leaving Mt. Hamilton. The place of the star (1880) is:

$$\left.\begin{array}{ll} \text{R. A.} & 10^{h}\ 33^{m}\ 26^{s} \\ \text{Decl.} & +9^{\circ}\ 28' \end{array}\right\}$$

This pair should be carefully measured every few years for some time to come.

CHICAGO, Aug. 8, 1892.

THE DOUBLE STAR, Σ 1216.

S. W. BURNHAM.

The angular change in the components of this binary is about 70° since its discovery by Struve. No orbit has yet been computed, and as it is probable that an approximate period could now be found, I have collected all the measures I have been able to find, and give them below in proper order:

	°	"		
1825.20	109.5	0.53	W. Struve	1*n*
1831.24	115.2	0.45	W. Struve	1*n*
1837.60	130.5	0.46	Madler	—
1842.20	178.4	0.8	Madler	1*n*
1844.30	178.2	—	Madler	1*n*
1851.28	139.4	0.49	O. Struve	2*n*
1853.25	148.3	—	Madler	1*n*
1855.24	152.1	0.57	Madler	2*n*
1857.34	150.0	—	Secchi	1*n*

	°	"		
1865.28	151.4	—	Secchi	1*n*
1865.48	166.5	0.56	Engelmann	5*n*
1866.55	151.5	—	Dembowski	14*n*
1867.32	131.5	—	Harvard Obs.	5*n*
1873.13	165.3	—	Brunnow	1*n*
1874.16	165.3	—	Wilson & S	2*n*
1874.18	167.0	—	Glenhill	1*n*
1875.27	165.5	—	Wilson & S.	2*n*
1877.18	164.6	0.35	Schiaparelli	2*n*
1878.20	158.8	0.61	Burnham	4*n*
1878.47	165.4	—	Cincinnati	5*n*
1879.21	160.8	0.37	Burnham	2*n*
1879.24	170.0	0.57	Schiaparelli	2*n*
1880.22	166.4	—	Cincinnati	1*n*
1880.32	167.6	—	Seabroke	2*n*
1880.91	166.7	0.36	Hall	3*n*
1887.24	173.8	0.45	Schiaparelli	5*n*
1891.26	183.2	0.46	Hall	4*n*

This star is Lalande 16375, and its place (1880) is:

$$\left.\begin{array}{ll} \text{R. A.} & 8^h\ 15^m\ 15^s \\ \text{Decl.} & -10^\circ\ 13' \end{array}\right\}$$

The measures by Madler in 1842-44, which are credited to this pair in the *Dorpat Observations*, Vol. XI, evidently belong to some other pair. Madler's observation of 1837 I have not seen in the original publication. It is included here on the authority of Secchi.

CHICAGO, Aug. 8, 1892.

NOTE ON THE MOUNT HAMILTON OBSERVATIONS OF MARS, JUNE-AUGUST, 1892.*†

EDWARD S. HOLDEN

Agreeably to the request of the Editor of ASTRONOMY AND ASTRO-PHYSICS the following paragraphs relating to the Mount Hamilton observations of Mars in 1892 (which are of course not finished at the date of writing, August 18), are presented. In merely describing the work done so far I am more or less speaking for my colleagues. They are, however, not responsible for the opinions expressed, naturally. Although the situation of Mars in this opposition is very unfavorable it was desirable to obtain as many observations as possible. The altitude of the planet ranges from about 28° to about 32° above the horizon

* Communicated by the author.

† The present note may serve a useful purpose in correcting certain erroneous statements regarding our work which have been widely circulated and which require correction.

(May–September), which is too low for satisfactory images of so difficult an object, even at Mt. Hamilton. These altitudes were, however, six degrees greater than the corresponding altitudes in Southern Europe.

The weather has been favorable and no pains have been spared to obtain all that could be got. The large telescope has been regularly used on Mars on the nights of Saturday, Sunday, Monday, Tuesday and Wednesday of each week by Professors Holden, Schaeberle and Campbell of the Observatory, and by Professor Hussey of the Stanford University (who has been spending the summer here in special work), and on Friday by Professor Barnard. The 12-inch telescope was also employed by Professor Barnard on other nights. Thus Mars has been under observation with the great telescope on six nights of each week, generally for the whole night.

PHOTOGRAPHS OF THE PLANET.

In May some experiments in photographing the planet (enlarged five times) were made by Professor Campbell and myself. While the larger markings and the polar caps were plainly shown, it was soon found that drawings would give far better results for the purpose in hand than photographs. A series of such photographs in connection with a set of eye-drawings and measures would be extremely valuable as a means of fixing the longitudes and latitudes of the principal points on the planet's disc. A serious practical difficulty in this plan is that the photographic lens of the great telescope ought not to be put on or taken off after dark. When it is once adjusted to the telescope it should remain during a whole observing night, and thus after a few photographs had been made, the rest of the night would be useless, so far as measures and drawings of the planet were concerned. At the opposition of 1894 the altitude of Mars will be 61°, and satisfactory photographs can then be secured.

DRAWINGS OF THE PLANET.

The earliest drawings of the planet with the great telescope were made by Mr. Schaeberle and myself on June 16, and since that time several drawings each night have been secured by Messrs. Schaeberle, Barnard, Campbell and Hussey. I have myself examined the planet on nearly every observing night and compared its appearance with my previous drawings made in the oppositions from 1875 onwards. Up to the middle of August, considerably more than one hundred sketches have been secured.

Some of these are very beautiful and complete. Most of them are still in the observing-books, but it is intended to copy them all on to forms of a uniform size and to publish what is important, after it has received a suitable discussion at the hands of the observers. Most of the drawings with the 36-inch have been made with a magnifying power of 350 diameters; a few with a power of 260. The planet has occasionally been examined with 520, but the air has not once been steady enough to employ this power throughout the night with advantage. How unfavorable the circumstances have been can be estimated when it is remembered that powers of 1000 and even more have been employed on Jupiter and Saturn with good results.

EXPERIMENTS ON THE CONDITIONS OF THE BEST VISION.

Various instructive experiments on the condition of the best vision have been tried. The planet has been viewed in the morning and evening twilight; through colored shade glasses; with diminished apertures; in an artificially illuminated field as well as in a dark one. As the color-curve of the telescope shows that the focal-points will be different for rays of different colors, (see *Publications* A. S. P. Vol. II, page 160), I have tried the experiment of looking at the dark canals, for example, with a focus suitable for the best vision of the larger markings on Mars which are of the same dark color. The theory of this process appears to be correct, but I have not noticed any material improvement in the vision.

MEASURES OF THE SATELLITES.

Whenever it would not interfere with more important work the satellites have been referred to the centre of the planet, by micrometer measures of position and distance. These measures have usually been made by Messrs. Schaeberle and Campbell. It is to be noted that most of these measures have been made when the planet was low, or the air unsteady, saving the moments of best vision for examination of the surface features. The measures have been made with the magnifying powers 350 and 520. They will be reduced and published shortly.

ECLIPSES OF PHOBOS.

All the eclipses of Phobos occuring during the periods of observation have been observed, usually by Messrs. Shaeberle and Campbell. The time of disappearance of the satellite in the shadow of the planet could be very accurately noted. The error

of estimation was probably not more than two or three-tenths of a second of time. The observation of these eclipses will be extremely valuable in fixing the elements of the satellite's orbit.

RELATIVE BRIGHTNESS OF THE SATELLITES.

At the request of Professor Hall we have made estimations of the relative brightness of the two satellites. Phobos is, of course, considerably brighter than Deimos. The observations are not yet reduced.

MEASURES OF THE INCLINATION OF THE PLANET'S AXIS.

Measures to determine the position of the axis of Mars have been made on many occasions, usually by Messrs. Schaeberle and Campbell. Like all other micrometric measures, they have been made at times when the vision was inferior, reserving the times of best seeing for an examination of the planet's surface. Some measures of the diameters have also been made.

MEASURES OF THE SIZE OF THE POLAR CAP.

These measures have been made so frequently that they will give a complete account of the decided and remarkable changes in the size of this marking. The polar-cap has diminished in size with the advance of the summer of Mars. It is worth inquiry whether a cap composed of dense clouds would not exhibit changes similar to those to which a snow or ice-cap would be subjected; or rather to those which have actually been observed.

SMALL STARS NEAR THE PLANET.

With a telescope so powerful as the 36-inch, small stars of something like the brightness of the satellites are frequently seen. All cases of such objects which might be new satellites have been carefully noted when they were fairly near to the planet.

OBSERVATIONS OF BRIGHT PROJECTIONS ON THE TERMINATOR OF THE PLANET.

A very interesting series of observations of bright projecting points on the terminator of the planet was begun by Mr. Schaeberle and myself early in June and was continued till the middle of July, and will be resumed (no doubt) towards the end of August. Measures to fix their positions have been made. During the opposition of 1890, similar observations seemed to show that these projections were the prolongation of white streaks on the planet (clouds?), (see *Publications* A. S. P., Vol. II, page

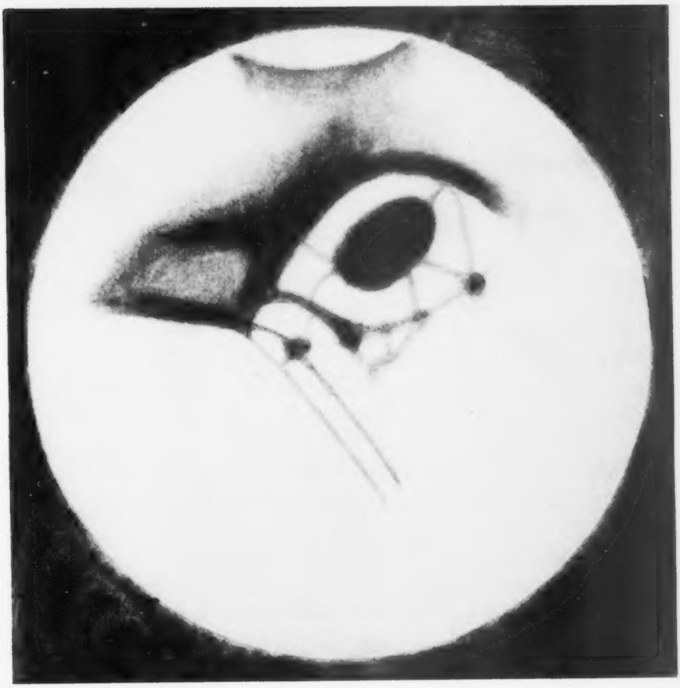
PLATE XXVIII.

$\lambda = 111°$

Mars, 1892, Aug. 14, 11^h 15^m P. S. T.

W. W. CAMPBELL.

PLATE XXIX.



$\lambda = 84°$

Mars, 1892, Aug. 17, 11^h 15^m P. S. T.

W. J. Hussey.

248), and the phenomena of 1892 have been examined to see if a similar conclusion would hold. The results have, so far, been indecisive.

CHANGES ON THE PLANET.

It is not practicable to describe the remarkable changes that have been noted on the surface of the planet without a set of copies of the drawings for reference. There have been very many such. Marked changes have occurred in certain regions during the present opposition (notably in the polar cap, in the region north and east of Lacus Solis, in the Fons Juventæ etc.).

In many cases a feature has remained tolerably constant during the whole opposition but is markedly different in its present form or color from the representations made in former years (notably the region about Fons Juventæ, the Lacus Solis, again, etc. etc.).

In some instances we have change of form alone; in others change of color; in a few cases the changes of form and color seem to be associated.

Nothing more definite can be stated in this regard until a series of published drawings is available to which references can be made.

I am able to enclose with this, however, copies of sketches which these gentlemen have copied for me, Messrs. Campbell and Hussey, and which I hope can be published to accompany this note. They are direct copies from the observing books and, like all other such drawings, have been made absolutely independently without any consultation between the observers. In general, only the parts of the planet's disc which are nearest the centre, and therefore best seen, are drawn. A combination of the hundred or more excellent drawings of this class ought to give some material additions and improvements even to the best existing map—that of Professor Schiaparelli.

SCHIAPARELLI'S CANALS AND DOUBLE CANALS.

Up to the middle of August many of the canals of Professor Schiaparelli were mapped. None were seen double until the night of August 17, when Professors Schaeberle, Campbell and Hussey made three entirely independent drawings each of which shows the canal marked Ganges on Schiaparelli's map to be distinctly double. Thus the Lick Observatory has the pleasure of confirming the discovery which Professor Schiaparelli made in 1881. I am especially glad to announce this fact as recent dispatches pur-

porting to be sent from Mount Hamilton "announced" that those double canals (which however had been seen here during the opposition of 1890), did not exist.

CONCLUSION.

I may briefly state my individual conclusions, as derived from a comparison of my own observations of Mars at the opposition of 1875 and at all succeeding ones, to be that the changes in the surface-features of Mars as we now know them are probably not capable of being completely explained by terrestrial analogies. What are we to make of the lake called Fons Juventæ, for example, which was a single object in 1877, which was not visible in 1879, and which has been both single and double during the present year?* The dark areas on Mars may be water and the red areas land; but how are we to explain the faintly colored areas like Hesperia, or Deucalionis Regio? Are they vast shoals like the Grand Banks of Newfoundland? Are they solid land, or are they water?

Is it conceivable that an observer on Mars, examining the earth in any part of its recent history, would have seen any such amazing topographic changes as we have this year observed, not to speak of the changes from opposition to opposition? It appears to me that a careful examination of the long series of drawings of Mars which we owe to Professor Schiaparelli and to others, up to the present time, will make it evident that there are enormous difficulties in the way of completely explaining the recorded phenomena by terrestrial analogies unless we also introduce serious modifications.

LICK OBSERVATORY, August 18, 1892.

MARS.†

WILLIAM H. PICKERING.

In my last paper upon this planet an endeavor was made to show that actual changes did occur upon its surface, besides the well known annual change in the size of the snow caps. This effort has perhaps proved unnecessary since the changes which have actually occured at the present opposition have been so conspicuous and startling that they might easily be detected even

* It became single just at the time of the appearance of the double canal through it.

† Communicated by the author.

by the possessors of six-inch telescopes. The canals can now be observed readily any evening. Many of those that we have seen here agree with Schiaparelli's, and several do not. Several of his more strongly marked ones have not been found at all. This, however, I am quite prepared to attribute to seasonal changes. Some very well developed canals cross the oceans. If these are really water canals and water oceans, there would seem to be some incongruity here. When the snow melts, it seems that there really should be some oceans, and a careful study has been made of the dark spot previously referred to, at the northern end of the Syrtis major. Although sometimes dark gray, yet in the great majority of cases when the seeing is satisfactory, and the spot is central, it appears of a clearly defined dark blue color. Another spot presenting a precisely similar appearance occupies a portion of the Sinus Sabæus or Herschel Strait.

These two spots when near the limb have on several occasions been observed to be of a beautiful bright blue color. If they are really oceans, they must, under these circumstances, be reflecting to our eyes the color of the Arean atmosphere, as water would, under similar conditions, do upon our Earth.

Viewed with a double image prism these spots when near the limb seem to present faint traces of polarization, the plane being radial to the planet. Until very recently they were much darker than any other spots visible, although a dark region near Solis Lacus (Terby Sea) has upon one occasion appeared quite black. It is my impression that these two areas are really water, and in the present article they will be referred to provisionally as the Northern and Equatorial Seas respectively. As I have stated in former articles I very much doubt if what are usually known as oceans and canals contain any water at all. That is to say, any water which is visible as such, for it is quite possible and perhaps probable that they may owe this color indirectly to the presence of water, stationary or running.

The boundaries of the Equatorial Sea (Fig. 2) are all sharply defined. It is 1300 miles in length, east and west, and averages a trifle over 200 miles in breadth, with two deep bays slightly curved, and almost precisely alike, opening southward, at its western end. In this article I have adopted the precedent set by Professor Schiaparelli in applying the terms east and west with the same signification as is given to them in maps of the Earth. That is they are reversed as compared with other celestial maps. Its total area is 275,000 square miles. The shape of the Northern Sea (Fig. 3) is that of an irregular quadrilateral, 750 miles

in length by 600 in breadth. On the north its outlines are as clearly defined as those of the other sea, but on the south it is bounded by a dark gray region, never seen hitherto to be blue and which I am inclined to ascribe for reasons which will appear later to low land. If its shores were indented, this might account for their rather indistinct appearance. Its area is nearly equal to that of the Equatorial Sea, being approximately 225,000 square miles. What we may therefore speak of as the permanent water area upon Mars amounts to about half a million square miles. This is exactly one half the area of the Mediterranean Sea. A glance at the map of the World in two hemispheres will give the reader an idea of the enormous disparity in the water area of the two planets. From this circumstance we might expect the climate of the smaller planet to be on the whole much the dryer of the two, and if all is not a desert, at least that the deserts would be much more prominent than upon the Earth.

In this connection we may refer to the green areas situated near the poles, and described in the June and August numbers of this periodical. It was then stated that after the vernal equinox the greens almost entirely disappeared and the question was raised whether the same effect would be noticed this year. We can now reply in the affirmative, for although we have searched for them with the utmost care of late, when the seeing was both better and worse than before, scarcely a trace of them have we been able to detect. There is also a green area to the west of the Equatorial Ocean, but this region we have not been able to inspect carefully of late. In case they should reappear before the present opposition is over, as is possible, it is hoped that others will be upon the watch to detect them, and accurately locate their positions. While their reappearance might with some show of probability be attributed to the presence of one of the great branches of organic life upon the planet, and with this branch, as an almost necessary corollary, the other one, we must still consider the matter merely in the light of a tentative hypothesis, until further observations are accumulated, and content ourselves with the statement that no facts have as yet been observed inimical to this idea. The one fact which we have so far attempted to demonstrate is the presence upon the planet of water in the liquid form, and the attempt has been made to determine its exact location, and the area and shape of the surfaces permanently covered by it.

As might have been expected from the position of the planet's axis, the snow cap is much more conspicuous at this opposition

than it was at the last. On June 23, the northern limit of the southern polar snow cap was, on the average, in latitude — 65°. This in our northern hemisphere would correspond to the latitude of northern Siberia, Iceland, and northern British America. As this date was but thirty days after the passage of the vernal equinox, it will be seen that the line of melting snow was rather nearer the pole than we might expect to find it upon on our own Earth at the same period. The area of this snow cap was some 2,400,000 square miles. Upon this date a small dark spot was noted near the center of the snow. The spot was then well developed, and must have been already existing for several days. Since that time it has grown rapidly, soon splitting the snow cap into two unequal parts, and of late changing its shape materially. The snow cap in the mean time has rapidly diminished in size, so rapidly in fact, that considering the weakened power of the sunlight at that distance, we are forced to believe that its depth is much less than that of the similar deposit covering the poles of our Earth. It will thus be seen that the comparatively small snow caps of Mars by no means necessarily imply a warmer climate than that of the Earth, as some writers have assumed, but merely a drier one. If the snow fell to a less depth, a larger proportion of the heat absorbed in the higher latitudes could be employed in raising the temperature, and a less amount absorbed in the latent form. This would involve a somewhat higher temperature during the summer, but a longer period of intense cold during the winter, than exists upon the Earth, in proportion to the length of the year.

Upon July 26 it was found that the area of the snow cap had diminished to 800,000 square miles. An area of 1,600,000 square miles of snow had, therefore, been converted into water, in the space of thirty-three days. With our extensive oceans this would produce no material change upon the Earth, but what must be the effect upon Mars, whose total permanent water area amounts to less than one-third of this figure? Moreover, upon the Earth the semi-annual transfer of the melted snow from pole to pole is conducted by means of the oceans, but upon Mars this transfer must take place across the land. We should naturally expect that a considerable proportion of the water would be absorbed or deposited upon the way. It will therefore be interesting to notice what has actually been observed.

Eastward of the stem of the Y mark, that is south of Libya, there was observed by Mr. A. E. Douglass upon May 8, and by myself quite independently, upon May 9, a light colored triang-

ular region with a bright triangular center (Fig. 1). The angles of the central region were so distinct that they were selected as stations for our micrometric survey of this surface. At the next presentation of this phase, a month later, the central triangle had entirely vanished, being of the same tint as the outer triangular area, thus rendering it quite impossible to employ the selected stations. The whole area was however still much lighter than the stem of the Y. June 11, it had a decidedly greenish gray tint when central, and two days later it had assumed the same gray color as the stem of the Y from which it was indistinguishable. July 17, that portion of this region south-east of the Northern Sea had become extremely dark (Fig. 4), being only exceeded in tint by the sea itself, which differed from it mainly in color, the sea being blue and this region gray.

Upon July 10, the region south-west of the Equatorial Sea was extremely faint, and but little darker than the reddish region to the north of it. A similar effect had been suspected in June. This seems the more singular, since after the Seas this is usually one of the very darkest and most conspicuous markings upon the planet. The region west of this has also been subject to various changes, which need not however be described in the present article.

Upon May 12, it was noticed that the southern snow cap was bounded by a very fine black line. By June 23 this had become quite conspicuous in some places. By July 10, that portion of the line lying upon the Arean meridian was as dark as the Equatorial Sea, and appeared quite like it. On July 16, a small elongated black spot was noticed upon the western side of the stem of the Y (Fig. 3). It was then so conspicuous, that I was surprised I had not noticed it before. My measurements indicated that it was about 125 miles in length by 75 miles in breadth. This would make it of about the same size as Lake Erie, and it was connected with the Northern Sea by a very narrow straight black line. This line did not at all resemble the so called canals, being much finer and blacker. This spot was again seen by myself upon July 17, and by Mr. Douglass upon July 22, after which it disappeared unexpectedly in a way which I shall presently relate.

Changes were now coming thick and fast upon the planet, and when evening came round, and we put our eyes to the telescope, we never knew what we should see next. In my August paper reference is made among other suspected changes to the two arms of the Y, which in the opposition of 1890 were always

PLATE XXX.

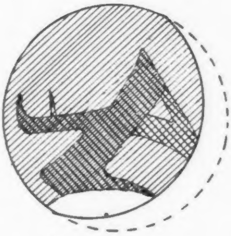


FIG. 1.
May 9, 21ʰ 05ᵐ

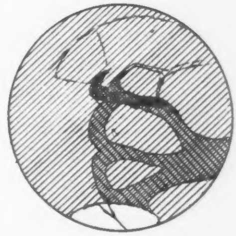


FIG. 2
July 14, 16ʰ 50ᵐ

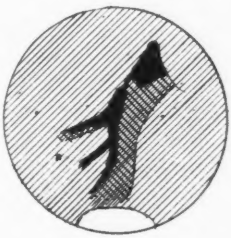


FIG. 3.
July 16, 17ʰ 45ᵐ

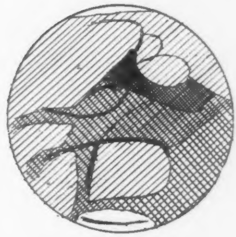


FIG. 4.
July 17, 15ʰ 50ᵐ

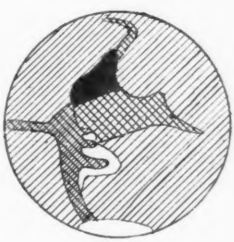


FIG. 5.
July 23, 17ʰ 30ᵐ

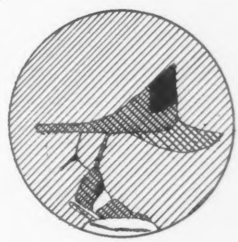


FIG. 6.
July 25, 20ʰ 40ᵐ

drawn of approximately equal width. The statement was then made: "At present there is no doubt but that the eastern arm is much the wider of the two, perhaps twice as wide." This paper was completed May 13, 1892. This statement still remained true upon June 10 and 11, but at the next presentation upon July 12, a central arm was shown, converting the Y into a trident. This arm connected directly with the dark streak or split in the snow cap (Fig. 2). The eastern arm was still much the widest, but in two days the difference between it and the western was much less marked, and by July 17, they were equal in breadth, just as they appeared in 1890 (Fig. 4). In the mean time the central arm of the trident had become much more prominent being about equally conspicuous with the other two, and now, to my astonishment was seen a large dark area south-east of the Northern Sea and of fully double its area. This dark region is the one referred to earlier in this paper as having formerly been very light colored. It was now nearly as dark as the Sea, and much darker than that part of the Y to the south of it. In color it was gray, and not blue. This observation was independently confirmed by Mr. Douglass the next evening. By July 23, this darkening had greatly diminished, the color of the dark region being of the same depth as that of the rest of the Y (Fig. 5), which latter had now materially changed its shape, owing to eastward extensions of the eastern arm: In the mean time the central arm, recently so strongly marked, had completely disappeared. But what was most extraordinary was that the Northern Sea had now extended far to the south-west, completely concealing the little lake and the channel connecting the two. This result was also confirmed independently by Mr. Douglass the next evening. By "independently" I mean that he made his drawing without having seen mine, or knowing at all what I had seen. Indeed, both of us were doing so much observing at this time that we had little opportunity to compare results, and unfortunately, did not fully appreciate the extent of the changes we were observing, and so devoted a considerable share of our attention to other matters. This will account for the apparent breaks in this record, for, with the exception of July 9, when some repairs were being made upon the telescope, continuous observations have been maintained since July 4.

To return to the observations, it is not clear from the record, whether the southern extension of the Northern Sea was blue or gray. It was merely recorded and drawn "as dark as the Northern Sea." On July 24 Mr. Douglass also recorded a large south-

ern dark spot which appeared to him as dark as the Northern Sea, but which I had not noticed upon the 23d. Upon July 25 the original outlines of the Northern Sea were again well seen (Fig. 6), the region south-west of it now being much lighter colored. The southern dark area seen by Mr. Douglass, and of which he had told me, was also noted. As a whole this area was not now as dark as the Northern Sea, but it contained a smaller spot which seemed quite as dark. There was also a narrow white channel extending northwards from the snow. The eastern arm of the Y, formerly so wide, was now reduced to a mere thread, while a trace of the central arm was again visible.

The Y is now so placed that it is only visible to the observatories to the west of us, and we shall not be able to observe it again until the middle of August. A striking difference may be noted in the arrangement of the dark channels in figures 3 and 4. In both instances they were well seen, and carefully drawn, and I do not see how the difference could be due to an error. The latter arrangement was subsequently confirmed by two other drawings. Regarding the former I find the record, "The dark parts are usually not more than 150 miles broad." I can scarcely think, however, that they could have been as broad as that.

The central branch of the Y was only noted by me upon one occasion in 1890, and that was upon May 25, when it was extremely faint. The date corresponding to July 12, 1892 in the previous opposition was August 24, 1890. At that time the Y was not visible in Cambridge. The corresponding date at the next opposition will be May 31, 1894. If the appearance of this central branch is in any way connected with the seasons upon Mars, it will be of interest for those observatories which are favorably situated at that time to look for it, since, should it then be as conspicuous a phenomenon as it has been this year, it could be readily detected by comparatively small telescopes.

In seeking to explain these observations, I would merely point out the fact that the changes occurred at a time when the snow was melting with great rapidity, that a dark channel suddenly appeared July 12, which had not been seen at the last previous observation of this region June 13, that it shortly disappeared again, and that a few days after this event the Northern Sea largely increased in area temporarily, or at least that its southern shores became much darker. I think these changes cannot be explained by Arean cloud effects. We have already observed large whitish patches upon the planet, which undergo

considerable changes in shape and extent from night to night. We are now studying them carefully, although we find them rather difficult of observation. These changes we are inclined to refer to clouds, although the matter is not so simple as it might at first appear. If these effects are really due to clouds, they are quite different in character from the other changes noted above.

If the reader is inclined to be surprised at the extraordinary character of the phenomena now apparently occurring upon our sister planet, as revealed by the telescope, I can assure him that he is no more so than were the observers themselves. Nor do we insist upon any explanation of these changes, but only upon the accuracy of the observations themselves. Owing to our remote and isolated position, we know nothing at the present writing of what has been done and seen at the northern Observatories, and it is possible that when this strikes the reader's eye, it will not be as new to him as it is new to us. Nevertheless, I am inclined to think that owing to our splendid atmosphere, and southern latitude, portions of what precedes may still be new, although the larger northern telescopes will doubtless have detected all the more important changes.

AREQUIPA, PERU, August 1, 1892.

Note in Regard to the Figures. In the above figures north is placed at the top. The date is given in Greenwich Mean Time. The scale is $\frac{1}{200000000}$ or 200 kilometers (125 miles) to the millimeter. In the last five figures $1'' = 1.4$ millimeters.

OBSERVATIONS OF MARS AT THE HALSTED OBSERVATORY, PRINCETON, N. J.*

BY PROFESSOR C. A. YOUNG, HALSTED OBSERVATORY.

The planet was observed by me at Princeton with the 23-inch telescope on every available night between July 6th and July 28th, but owing to interruptions by bad weather and other causes the actual number of satisfactory views obtained was not large. On about half a dozen occasions, however, the seeing was good enough to permit the use of magnifying powers of from 500 to 700, and on the two nights of July 23d and 25th it was especially fine.

The most conspicuous feature upon the planet's surface was the

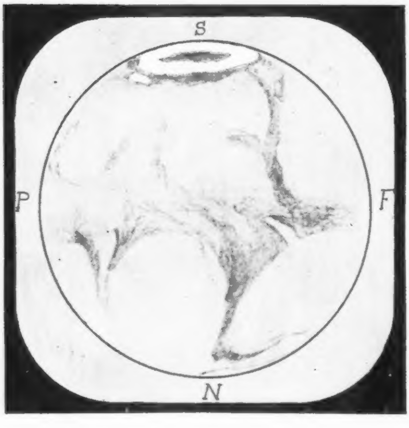
* Communicated by the author.

great southern ice-cap: on July 6th it measured very nearly 10″ across—about 1900 miles,—but it melted away rather rapidly, and on July 25th was not more than 1200 miles across by estimation. At first it was dazzlingly brilliant, and uniformly so over its whole extent, but on the 23d I noticed a dark streak in it, as if it had melted away at the centre: the length of the streak was about a third of the diameter of the cap, and at 11^h 45^m P. M. (Eastern Standard Time) its direction was, as nearly as I could judge, perpendicular to the central meridian. No satisfactory estimate of its width could be made of course, on account of its nearness to the limb; but that it was a "streak," and not in reality nearly circular, was evident from the fact that two hours later it had obviously changed its direction and was no longer perpendicular to the meridian. This central melting of the southern ice-cap is quite in accordance with the appearance indicated upon Schiaparelli's map of 1877, which shows an elongated patch entirely one side of the pole.

The edge of the cap was separated from the general surface by a rather conspicuous dark streak, which was by no means uniform in width. On the 25th two small bright patches were noted just at the edge of the cap; one in areographic longitude of about 300° (probably Schiaparelli's "Novissima Thyle"), and the other about 210° ("Thyle II"). On the night of the 24th the planet was observed for a few minutes with the 9½-inch telescope, and I was able to see the central dark spot in the southern cap, though only with difficulty.

The appearance of the planet in general corresponded much more closely to the drawings of Green, made at Madeira in 1877, than to any others with which I am acquainted. The principal features near the planet's equator were well seen at different times; especially the Syrtis magna (or Kaiser Sea) and its surroundings on July 23 and 25. Of course I tried very earnestly to make out the 'canals,' which figure so conspicuously in Schiaparelli's maps, but mostly without success. There were, indeed, various faint markings some of which with a low power seemed to correspond fairly in position and general direction with 'canals' shown upon the map; but under higher magnifying powers the resemblance disappeared; that is, instead of being narrow lines well defined and nearly straight, they became mere shadings, irregular, indefinite and vague in outline, and often discontinuous. But I should not like to be understood as denying the reality of the features described and depicted by the Italian astronomer. The 'seeing,' though it was unusually fine for Princeton on the

PLATE XXXIII.



Mars July 26, 1892, 6:30 A. M., Greenwich Time.

OBSERVED AT PRINCETON, N. J., WITH 23-INCH TELESCOPE.

23d and 25th, was probably not equal to that which prevails in the Italian atmosphere, nor can I pretend to any remarkable keenness of vision. I can only say that my observations 'failed to confirm' those of Schiaparelli, and left me rather skeptical.

The continuation of the northern extremity of the Syrtis Major which bears on the map the names of Nilo Syrtis and Nilus, was however clearly traceable; also certain short dark streaks in the southern continent, corresponding, perhaps, to the 'canals' marked Xanthus, Scamander, and Simois. At the upper (southern) portion of the Syrtis magna, the curious curved bright streak which bears the name of Oenotria was conspicuous, and also certain others, less brilliant and more indefinite in outline, but in a general way resembling it in their direction and curvature—perhaps the Ausonia and Libya of the map.

On July 6th at midnight the region of the 'Lake of the Sun' (Solis Lacus) was nearly central; the seeing was not remarkably good, but the principal features of the region could be made out, in pretty close accordance with the drawings of Green already referred to. On the evening of the 14th, the 'forked bay' (Sabæus Sinus) was in good position, and well seen; but no trace of canals could be detected, though the whole disc was covered with a mass of beautiful detail of varying shade and color, quite defying my power of delineation.

I send a somewhat enlarged copy of a sketch made 1:30 A. M. of the 26th (E. Standard time). It does not profess any minute accuracy of detail, but shows fairly well the principal features as they appeared to me. The preceding limb was of course somewhat shaded, while the following one was very bright, especially near the southern ice-cap.

The satellites were always, when not behind the planet or its disc, visible even with the planet in the field; and they were conspicuous when it was put behind a shade of neutral glass cemented to the field lens of the eyepiece. Several sets of micrometric measures were made, but they have not yet been reduced. Deimos usually ceased to be visible at any distance from the planet's limb less than 10″. Phobos on the other hand could be seen when within 5″ of it. So far as could be judged before final reduction of the micrometer observations, the ephemeris of the satellites given by Hall's tables is almost absolutely accurate.

On the night of July 23-4, the planet occulted a small star of about the 10th magnitude nearly centrally, the positive angle of the point of contact being about 120°. The star was last seen, nearly, but not quite in contact, at $1^h\ 43^m\ 15^s$ (July 25 A. M.),

and I judge the actual contact was about 15 seconds later; the seeing at the time was poor, so that it was not possible to make out any changes of form or color due to the planet's atmosphere.

I was obliged to leave Princeton on July 29th. Observations were kept up until Aug. 15th by my assistant, Mr. Reed, but he reports nothing new or of special interest.

HANOVER, N. H., Sept. 7, 1892.

MEAGER NEWS FROM MARS.*

LEWIS SWIFT.

The observations of Mars made at this Observatory during the present favorable opposition have been a series of disappointments, save on two or three nights when the sky was clear; the seeing, from atmospheric disturbances, has been unusually poor. At almost any opposition for many years I have, I think, seen more of detail with my 4½-inch comet seeker than I have at this time been able to secure with my 16-inch telescope. The great southern declination of the planet will, of course, account in part for this, but, in my opinion, not wholly.

On the evening of July 31 both Professor Todd, then my guest, and myself saw both the satellites, and on two occasions since I have been able to see them though not both at the same time.

The snow zone has been steadily decreasing and is now too small to be easily observed with small instruments.

The most important of the observations I have made was that of a small, black, circular spot, one-half of which was superimposed on the following side of the snow zone, the remainder being outside, the edge of the zone cutting the spot through the center. Though I saw this on three occasions, yet it was visible only during moments of good definition. I judged it to be equal in size to the shadow of Jupiter's smallest satellite. Its cause I can ascribe only to a denudation of the land from snow by the heat of his Antartic summer. Besides myself, this spot was also seen on one evening by a visitor at this Observatory.

Though carefully sought for, nothing resembling canals, single or double, has been observed. Previous to the opposition a rather large dark spot, quite irregular of outline, was seen directly under the snow-cap but became invisible at and near opposition.

I have observed in nearly all my studies of this body since

* Communicated by the author.

opposition, a large darkish spot, resembling in appearance and shape a bear-skin rug, which covers a large portion of the planet's disk.

Except these perhaps inconsequential notes, nothing worthy of extended remark has been viewed. This opposition has furnished to northern observers no new evidence as to the planet being an inhabited world.

WARNER OBSERVATORY, Rochester, N. Y., Sept. 1, 1892.

OBSERVATIONS OF MARS AT THE WASHBURN OBSERVATORY.*

GEO. C. COMSTOCK, WASHBURN OBSERVATORY.

Two series of observations of Mars have been made at Madison during the present opposition in addition to a careful examination of the disk of the planet and such occasional measurements as were suggested by its appearance.

I. A series of meridian circle determinations of the declination of the planet and neighboring stars in accordance with the programme issued from the Naval Observatory by Professor Eastman. The programme has been rigorously followed with the exception that owing to delay in securing a reversing prism the early part of the observations were necessarily made without reversal of the images. The later observations will, however, furnish abundant material for the determination of any systematic differences arising from this cause. Between June 25 and Sept. 9 the planet was observed on forty-five nights and the observations will be continued up to Sept. 23. It would be premature to attempt to state at present any results of this series of observations.

II. With the 15½-inch equatorial telescope a series of measurements of the position angle of the south polar cap on the planet has been made for a determination of the position of the rotation axis of Mars. In order not to interfere with the meridian circle work the observations were usually made about an hour before and after the meridian passage of the planet and, in order to avoid, as far as possible, the effect of defective illumination of the disk, they were confined to a period of about three weeks preceding and following the opposition. During this period fifty-four observations upon twenty-nine nights were secured, and although the definitive reduction of the observations is not yet completed, a

* Communicated by the author.

provisional discussion indicates a considerable correction, approximately — 2°, to the position angle of the axis given in Marth's ephemeris.

Especially noteworthy is the small polar distance of the center of the cap when compared with previous determinations as shown in the following summary, for the major part of which I am indebted to Houzeau's *Vade-Mecum de l'Astronomie*:

Date.	Observer.	Polar Distance.	Longitude.
		°	°
1783	W. Herschel	8.1	
1830	Bessel	8.1	
1837	Beer and Maedler	8.0	
1858	Secchi	17.7	
1862	Kaiser	4.3	192.3
1877	Hall	5.2	20.7
1877	Schiaparelli	6.1	29.5
1892	Comstock	1.6	8.2

The Areographic longitude of the center of the spot in 1892 differs but little from that determined in 1877, but both of these determinations are widely different from that of 1862, as is shown in the last column of the summary above. These numbers appear to indicate a considerable change in the position of the cap from year to year, but the anomalous result obtained by Linsser, who estimated a polar distance of *about 20°*, simultaneously with Kaiser's determinations of 4°.3, indicates very great possibilities of systematic error inherent in the observations.

Concurrently with the determinations of the position of the polar cap a series of measurements of its diameter was made and these will be continued as long as the cap admits of accurate observation.

PRELIMINARY REMARKS ON THE OBSERVATION OF MARS 1892, WITH THE 12-IN. AND 36-IN. REFRACTOR OF THE LICK OBSERVATORY.*

E. E. BARNARD.

My observations of Mars have been confined principally to the 12-inch. Since July 1st I have been able to observe the planet once a week with the 36-in. The greatness of the intervals between the observations with the large telescope has prevented in many cases verification of important details, since different portions of the planet's surface were presented at the weekly observa-

* Communicated by the author.

tions, and no one portion of Mars was ever under examination at successive observations. A bad night, which several times occurred, still further lessened the value of the work with the 36-inch. From these circumstances my own work at this opposition is not as satisfactory to me as I should wish.

I have carefully avoided putting anything on record that was not certainly seen, and this may account in the main for any lack of detail in my drawings. What is shown, however, can be relied on as having been seen and will in general be found fairly well located.

In observing the planet I have found that the lower powers were much more preferable. With the 36-in. 260 diameters was best for details but the measures have all been made with 520 diameters. Some of the stronger details have been more satisfactory with the higher power. An intermediate power of 320 has also been usefully employed.

With the 12-inch a power of 175 has shown the details best, but the measures, as with the other instrument, were made with a much higher power.

I have paid strict attention to the polar cap which has shown many singular and interesting phenomena. The diameter and position angle of the southern cap (the northern one was never visible) has been repeatedly measured with both instruments in the hope of detecting the relation between its decrease and the Martian season. There was a pretty regular decrease until the last of August when the cap rather rapidly diminished to a very insignificant light speck scarcely distinguishable from the limb. So far the cap has decreased from 10″, about the last of June, to about 3″ in the first part of September. The actual decrease has been somewhat greater than this. Reduced to the time of opposition these values become 12″.4 and 3″.5 respectively. The area of the cap during the above interval has diminished fully nine-tenths. If this is snow and ice, and every thing seems to point that way, and the water therefrom is distributed in the equatorial regions, it would almost suggest a possible oscillation of the axis of rotation due to the transportation of vast masses of water from the poles to the tropical regions as the seasons progress.

I believe it has been suggested by some one that large dark diffusions apparently streaming from the polar cap equatorward, are water produced by the melting of the ice cap. Doubtless this is the sheerest fancy. There are, however, long dusky areas emanating in the cap and tending equatorward, which doubt-

less gave rise to the idea. But there are changes here that are so vast and so rapid as would hardly be warranted by the action of the Sun on the ice cap unless the ice and snow on Mars are very different from our own. In the latter half of June an irregular dark area appeared near the middle of the polar cap. On at least one occasion this was reddish like the so-called continents.

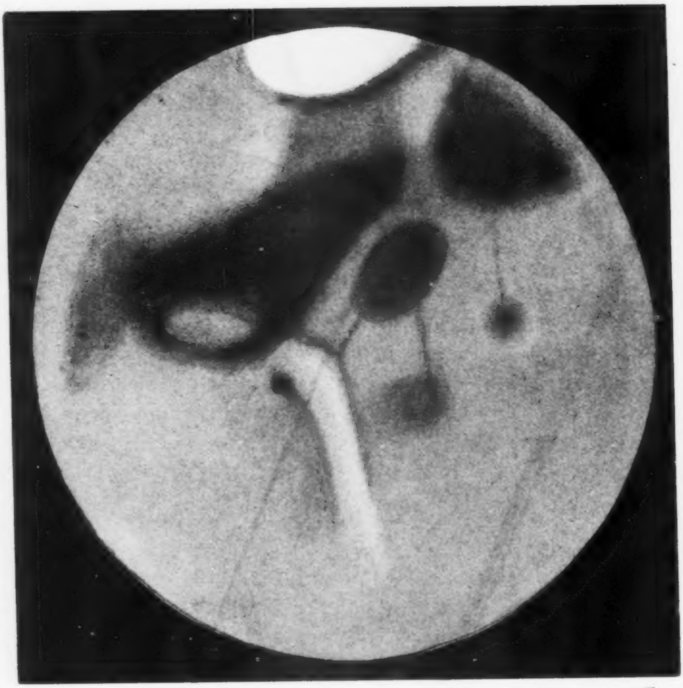
By the latter part of July the entire polar cap, presented to the earth, seemed to be heavily obscured and dusky, while two brilliant white spots appeared on it. It had, however, again resumed its brilliancy by the first week in August. On several occasions detached portions were seen lying close to the parent cap.

About August 19th an irregular portion or bay in the cap was brought into view by the rotation of the planet. By the 21st, this was seen to be abruptly terminated on its preceding side by a sharp notch and projection. At first there was no perceptible separation, but gradually a dark line crept across near the edge of the cap and finally separated a large mass from the main portion by a dark channel. This seemed to be the forerunner of rapid changes, for later, at the end of August near this same region, a large portion of the cap seemed to become obscured or dissipated. The entire cap rapidly diminished in size until a few days later only a tiny glimmer of the once brilliant cap remained.

On several occasions, the sharpness of outline, and the brilliancy of the cap has been almost startling in its vividness. At these times the color of Mars has appeared very strong—a deep rich orange.

There is, of course, a very great diversity in the depth of shade of the surface markings of Mars. Certain of the so-called seas are much darker than others, and there are some so faint as to be scarcely discernible—such are in the south polar regions. Their feebleness of tint is not altogether due to being seen at a great latitude. There is a great preponderance of markings and details south of the equator even to the pole, while to the north scarcely any detail could be made out. There are certain remarkable differences apparent between the present appearance of the planet and previous charts. If the charts are at all correct—and they doubtless were—important changes are at work on the planet. A short distance following the *Solis Lacus*—the Terby sea of some charts—is a small dark spot that is not shown on Schiaparelli's celebrated chart. This small spot seems to vary very much in depth of shade. It has been seen very dark and

PLATE XXXIV

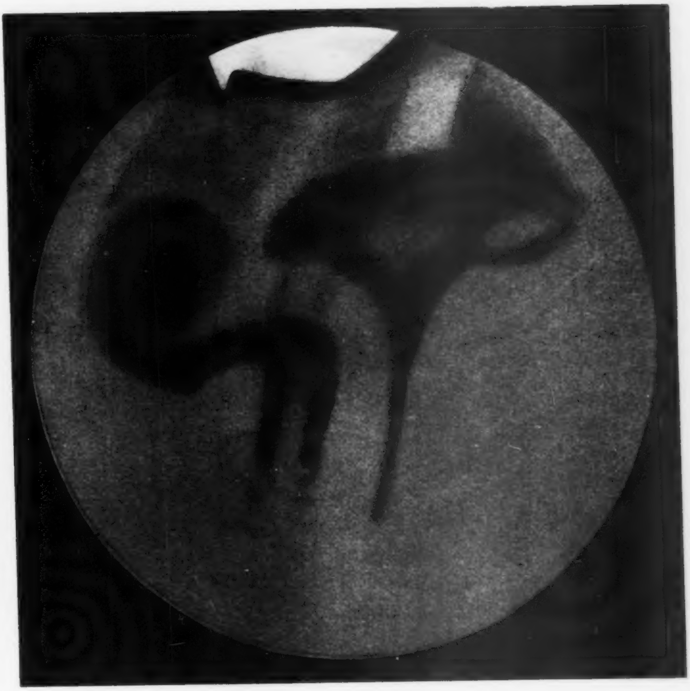


Mars, 1892, Aug. 19, $12^h\ 1^m$

36-in. Equatorial of the Lick Observatory.

E. E. Barnard, *Delt.*

PLATE XXXV.



Mars, 1892, Aug. 21, 8^h 58^m

12-in. Equatorial of the Lick Observatory.

E. E. BARNARD, *Delt*.

then again quite pale. It is connected with the great sea south by a slender thread-like line. There is a small canal running north from the *Solis Lacus* to a diffused dusky spot which does not appear on Schiaparelli's chart. The region about the Solis Lacus seems to differ much from Schiaparelli, while the lake itself is much larger than he has shown it.

There are other differences fully as marked as these at other points which will be discussed in a later paper. These striking changes are enough to make us pause and question whether what we see before us in the heavens is really another world like our own, with relatively fixed oceans and continents, or whether it is not a world like our own in its younger days when continents were shifting and oceans changing, before the surface of the earth became firm and fixed by the process of cooling. If the latter is the case we can quite readily decide that Mars is not inhabited by the higher orders of life.

The so-called continents are not uniform. Bright areas being rather frequent and long luminous streaks have been observed and appear to be as much a feature of the planet's surface as are the seas; if the observations of one opposition are to count, I have noticed only one object of a very transient nature—such as might possibly be a cloud. On August 3d a conspicuous luminous spot was visible in the 12-inch that I had not noticed before. It was small and elongated about 2″ or 3″ in diameter. It was in longitude 219°, latitude about 30° or 40° north. Careful measures were made of its position on the disc to be compared with subsequent observations of it. Though carefully looked for, I could not see it on succeeding dates. If it is a permanent feature, it must have been much brighter on the 3d.

I have measured the positions and observed the transits of a number of markings on the planet during the opposition.

The satellites have been carefully observed and measured on every possible occasion and estimates of their relative brightness made. These will presently be published. Phobos has been decidedly the brighter of the two at all times.

They have both been seen with the 12-inch. On July 8th a star was occulted by Mars—passing behind the planet at the polar cap.

I have not been able to verify the duplicity of any of the canals of Schiaparelli, though this phenomenon has been carefully looked for.

I send two drawings of the planet, one with the 36-in. and one with the 12-in., as specimens of what I have been able to see. In

the second of these the region shown in the first is just disappearing at the preceding limb. The following are the longitudes and latitudes of the center of the pictures:

$$\text{Aug. } 19, \lambda = 79^\circ.5 \qquad \beta = -11^\circ.7$$
$$\text{Aug. } 21, \lambda = 16\ .3 \qquad \beta = -11\ .7$$

MT. HAMILTON, 1892, Sept. 8.

OBSERVATIONS OF MARS AT GOODSELL OBSERVATORY.

H. C. WILSON.

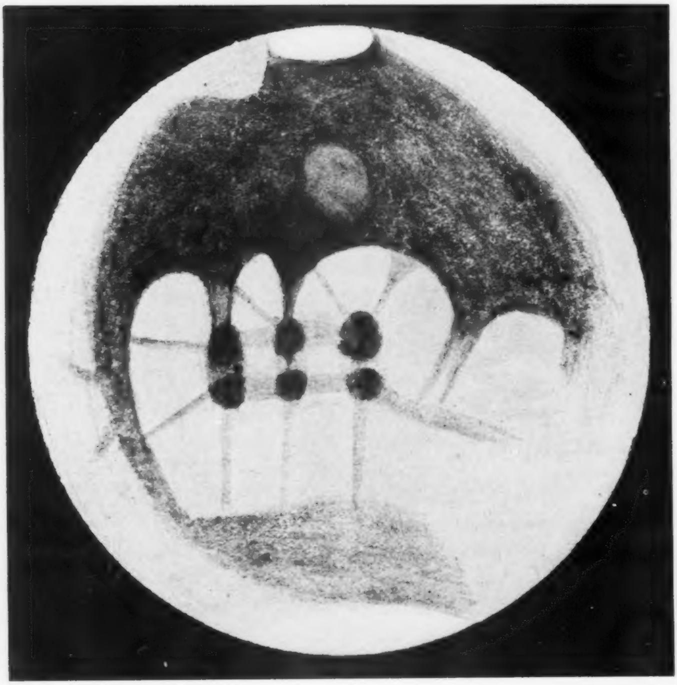
At Northfield the altitude of Mars during August was never greater than 22°, so that the seeing was seldom good. On a few nights, however, we were able to make out not only the prominent markings but some of the "canals" of Schiaparelli. These latter were extremely difficult to hold in vision for any length of time, but were at moments distinctly seen. Sketches were made at the telescope on ten nights. We reproduce two of the sketches (Plates XXXI and XXXII) made on the nights of best seeing, Aug. 13 and 26.

The satellites were always seen with comparative ease, when near elongation, when the planet was covered by an occulting bar. They could be seen without the aid of the occulting bar when the seeing was good enough to allow the use of a power of 800 or more. Phobos was always estimated brighter than Deimos.

The magnifying power which generally showed the markings best was about 300. When the seeing was poor it was sometimes improved by capping down the objective to 8 inches.

Up to Aug. 11 the only canals seen were Titan, Tartarus, Cyclops and Cerberus as one, Hephaestus and Propontis. On this date at 11^h the Solis Lacus region was near the center of the planet's disk. This region differs so much from Schiaparelli's map that it is difficult to identify the features drawn with those in the map. The area above and to the left of Solis Lacus, in which Schiaparelli has the canals Nectar and Ambrosia, is all nearly as dark as the so-called seas. Lacus Phœnicis is so large that in comparing the drawing with the map I at first mistook it for Lacus Solis. The canals Easphoros, Phasis, Sirenius and Eumenides and another connecting Lacus Solis and Lacus Tithonius are shown in the sketch. The same features are shown

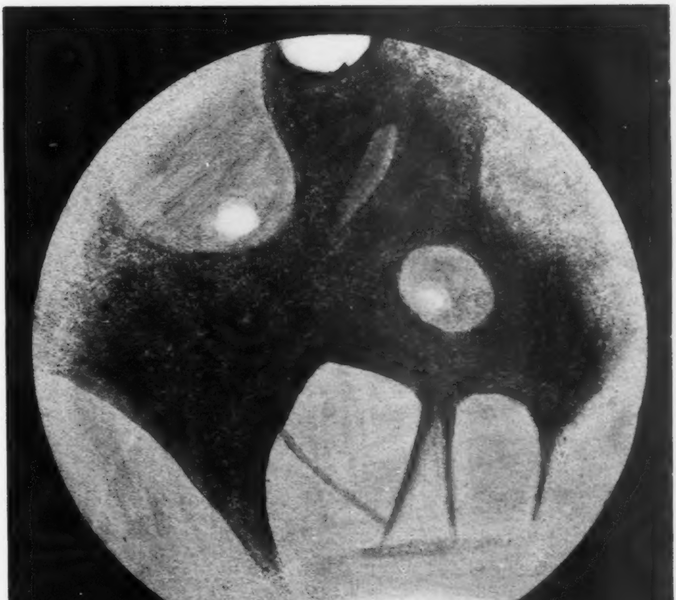
PLATE XXXI.



Drawing of Mars by H. C. Wilson,

at Goodsell Observatory, Northfield, Minn., Aug. 13, 1892, at 12h Central Time; Longitude of Center 102°.

PLATE XXXII.



Drawing of Mars by H C. Wilson,

at Goodsell Observatory, Northfield, Minn., Aug. 26, 1892, at 10ʰ 45ᵐ Central Time; Longitude of Center 328°.

with others on the drawing made Aug. 13 from 11^h to 13^m, which is reproduced in Plate XXXI. The seeing on this night was for a time excellent but became poorer before the drawing was completed. The most prominent markings were located in the drawing during the first half hour. The three double spots near the equator were seen by Professor Crusinberry, of Drake University, Des Moines, Ia., as well as by the author of the drawing. We may perhaps identify the right hand pair with Lacus Phœnicis, the middle pair with Tithonius Lacus and the others with Fons Juventæ and portions of Auroræ Sinus. The faint streaks it is difficult to identify with certainty with Schiaparelli's map, though the three parallel streaks running northward may be Ganges and Fortunæ and Iris. The doubling of these spots was noticed only at brief moments. The appearance was generally that of three single spots. The same spots were seen again on Sept. 14 and 15, when the definition was not good enough to allow the canals to be seen.

August 24 at 10^h the seeing was good and the appearance of the planet essentially the same as on the 26th at 10^h 45^m. The sketch for the latter date is reproduced in Plate XXXII. The streaks on the lower portion may be identified as Typhon, Hiddekel, Gehon, Deuteronilus and Oxus. The whole of Syrtis Major and Libya is drawn dark. The region Hellas is considerably larger than in Schiaparelli's map published in *l'Astronomie* 1889. In the lower part of this region was a small white area. Another white area was noticed in the lower left portion of Deucalionis Regio. In fact, in the moments of best seeing, the whole of the dark area on the planet seemed to be sprinkled over with very small cloud-like white spots. Noachis Regio appeared as a light streak to the right of Hellas.

The south polar cap has always appeared perfectly white and round (elliptic by projection), except on Aug. 26, when there was a suspicion of a slight notch in the edge as shown in the sketch. At the north edge of the disk a much larger white area, less well defined, was always seen.

RECENT OBSERVATIONS OF JUPITER—THE GREAT RED SPOT AND ITS CHANGES.*

E. E. BARNARD, LICK OBSERVATORY.

Many changes have taken place on the surface of Jupiter since last opposition. Three heavy conspicuous belts now cross the face of the planet. The distance of these belts from the northern limb, measured on July 31, were 13″.5, 18″.4, 26″.1. There are other and narrower belts, but these three are the leading features.

The detail in the Southern Hemisphere, as has been strikingly characteristic of that region for at least the last 12 years, is very much broken and irregular, consisting of strips, patches and spots.

The great red spot is still visible, but it has just passed through a crisis that seemingly threatened its very existence. For the past month it has been all but impossible to catch the feeblest trace of the spot, though the ever persistent bay in the equatorial belt close north of it, and which has been so intimately connected with the history of the red spot, has been as conspicuous as ever. For a while there was only the feeblest glow of warmth where the spot ought to be. This has been the case under the very best seeing. It is now, however, possible to detect a feeble outline of the following end and the feeblest traces of the entire spot. An obscuring medium seems to have been passing over it and has now drifted somewhat preceding the spot.

Among the interesting features of the northern hemisphere is a small, rather conspicuous black spot on the north edge of the northern of the three large belts. This object, which is similar to the small northern black spots of last year, is interesting since its motion seems to be about the same as that of the great red spot. Indeed the observations might imply a longer rotation period which would be very extraordinary as the red spot seems to be alone in reference to slowness of rotation.

Following are a few notes on the appearance of the great red spot this year:

July 24. There are only the feeblest traces of the great red spot. It is exceedingly faint and pale. The following end alone can be made out. The spot seems to blend into darker regions south and preceding it. Seeing = 5. As near as possible the

* Communicated by the author.

remnants of the spot transitted at $15^h\ 18.7^m$ Mt. Hamilton M. T., $\lambda = 357^\circ.2$.

July 29. Observed with the 36-inch. The following end of the great red spot in transit at $14^h\ 41^m$. There is a diffused pinkish glow where the red spot ought to be, but there is no form whatever to it. Seeing = 3–4. This observation would give $\lambda = 354^\circ \pm$ for the red spot's centre.

Aug. 3. The spot seems to blend into the general surface preceding which is dusky. There is only a feeble trace of the following end. At $14^h\ 0^m$ the following end is some 10^m past transit.

Aug. 5 with the 36-inch. $15^h\ 10.2^m$ the remnants of the red spot central. The corresponding $\lambda = 355^\circ.5$ The spot has no outline at all. There is nothing to show its presence but a feeble glow. There is a white spot on it near where the preceding end ought to be. Seeing = 5.

Aug. 8. $12^h\ 40^m$ red spot in transit. $\lambda = 355^\circ.9$. Seeing = 4. The spot is now a little more definite. It would seem that the cause of its almost total disappearance is passing away. The following end is faintly seen and the entire outline is vaguely made out.

Aug. 12. (36). Red spot central $15^h\ 59^m.4$ $\lambda = 357^\circ.3$.

The following measures of its position were made:

Center of spot from north limb = $29''.1$ (2 obs.).
" " " " south limb = $13''.1$ (2 obs.).

It will be interesting to watch the small white spot to see if it leaves the red spot. It resembled the ordinary white spots which have been so abundant south of the red spot the past few years.

If it is one of these it would show that the red spot is in a lower stratum than this one.

The small northern dark spot, previously mentioned, has been observed on the following occasions:

July 24. $14^h\ 55^m.8$. The small spot in transit and its longitude $343^\circ.3$.

Aug. 5, with 36-in. $14^h\ 56^m$. The small spot in transit $\lambda = 347^\circ.4$.

Aug. 8. $12^h\ 28^m$. The small spot in transit. $\lambda = 348^\circ.6$.

Aug. 12 (36). Small spot in transit at $15^h\ 47^m.9$. $\lambda = 350^\circ.4$.

On account of its high latitude, great exactness in observing its transits is not possible, and so the apparent increase of longitude may not be real.

On July 8—at transit, its position was measured with the micrometer.

From north limb 9″.3 (2 obs.).
From south limb 29″.7 (2 obs.).

The following observations of the satellites have been obtained. Mt. Hamilton Mean Time.

JULY 22. SATELLITE I LEAVING THE DISC.

1st contact	15^h	22^m	42^s
½ off	15	25	12
Last contact	15	28	7

JULY 29. SATELLITE I GOING ON THE DISC.

1st contact	15^h	$7^m.5$
½ on	15	10 .0
Last contact	15	11 .6

AUG. 5. SHADOW OF SATELLITE I GOING ON.

½ on	15^h	$44^m.2$

AUG. 7. SATELLITE I LEAVING THE DISC.

1st contact	13^h	$33^m.5$
½ off	13	35 .5
Last contact	13	36 .9

AUG. 8. SATELLITE IV GOING BEHIND THE DISC.

½ under	12^h	$54^m.3$

The satellite disappeared at a very high north latitute.

AUG. 12. REAPPEARANCE OF SATELLITE II FROM OCCULTATION.

½ out	16^h	0^m	14^s
Last Contact	16	2	29

Observations with the 36-inch. Seeing perfect. No trace whatever of the satellite through the edge of Jupiter. The limb of the planet cut the satellite sharply. II was very much brighter than the limb. I have previously observed several other occultations of the satellites with the 36-inch under good conditions but have never been able to see the least trace of a satellite through the planet, the limb always appearing perfectly opaque. A similar negative result has occurred in a great many observations of occultations with the 12-inch, and smaller instruments.

Aug. 14, 1892.

LEWIS MORRIS RUTHERFURD.*

JOHN K. REES.

In the death of this eminent man astronomical science has lost one of its most efficient workers; one who loved devotedly his chosen labor, and who did much to originate means and methods and gave most invaluable direction and impulse to astronomical photography.

Mr. Rutherfurd was born at Morrisania, N. Y., on November 25th, 1816.† John Rutherfurd, his grandfather, was of Scotch descent, and served as Senator of the United States from New Jersey from 1791 to 1798. He was also one of the commissioners for establishing the boundary lines between several states, and assisted in laying out a portion of New York City. The Earl of Stirling, Major General William Alexander, was an uncle of John Rutherfurd. General Alexander took a distinguished part in several battles of the Revolution. He is said to have been an excellent mathematician and somewhat of an astronomer. Lewis M. Rutherfurd's mother was a direct descendant of Lewis Morris, one of the signers of the Declaration of Independence, and after him Mr. Rutherfurd was named.

At the age of fifteen he entered the Sophomore class at Williams College, and was graduated in due course. At College he showed his love for investigation, and was made assistant to the Professor of Chemistry and Physics. He aided in the lecture preparations and experiments. Here he gave evidence of his taste for scientific work and of his mechanical skill. After graduation he studied law with the Hon. William H. Seward, at Auburn, N. Y., and was admitted to the bar in 1837. Mr. Rutherfurd's associates in the practice of law were the distinguished men, Peter A. Jay, and, after Jay's death, the Hon. Hamilton Fish.

During the practice of his profession he gave much of his leisure time to studies and experiments in chemistry and mechanics bearing on astronomy. In the early part of his professional career he married Miss Margaret Stuyvesant Chanler, a niece of Peter G. Stuyvesant. His wife's fortune added to his own made it agree-

* Communicated by the author.

† I am indebted for many items of information in this brief sketch to Mr. Rutherfurd's son, Rutherfurd Stuyvesant, Esq., of New York City, who at my request sent me a copy of some notes written out by his father a few years ago after urgent and repeated solicitation on the part of the son. A portion of these notes was used in preparing the article on Mr. Rutherfurd in Appleton's Cyclopædia for 1888.

able and possible for him in 1849 to abandon the law, and thereafter he devoted his leisure to science, principally in the direction of astronomical photography and spectrum analysis.

In 1849 he travelled in Europe and studied with the Italian optician Amici. He remained abroad some time travelling and studying. On his return to New York City he erected in his garden, at the back of his house at the corner of Second Avenue and Eleventh Street, a small but excellent Observatory. The building was arranged to contain transit instrument, clock and equatorial telescope. It was a very modest building but destined to be the witness of great deeds.

Near by, in the fine dwelling house, were a commodious study and a work shop fitted with turning lathes and tools of all kinds necessary for his work. The splendid work of Bunsen and Kirchhoff was then attracting great attention, and Mr. Rutherfurd devoted most of his time to spectroscopic investigations. One result was that in January 1863 he published in Silliman's Journal* a paper on the spectra of stars, moon, and planets. In this paper he gave diagrams of the lines and a description of the instruments employed. This paper was the first published work on star spectra. In this important communication the first attempt was made to classify the stars according to their spectra. He wrote in this paper, "The star spectra present such varieties that it is difficult to point out any modes of classification. For the present, I divide them into three groups: first, those having many lines. and bands, and mostly resembling the Sun, viz., Capella, β Geminorum, α Orionis, etc. These are all reddish or golden stars. The second group, of which Sirius is the type, presents spectra wholly unlike that of the Sun, and are white stars. The third group comprising α Virginis, Rigel, etc., are also white stars, but show no lines; perhaps they contain no mineral substance or are incandescent without flame."

The spectroscope of that day was a rude instrument, not well understood; and its results, of course, do not compare in definition and accuracy with spectroscopes of more recent times, but Mr. Rutherfurd's results were most suggestive and valuable.

In the course of his observations upon the stellar spectra he discovered the use of the star spectroscope to show the exact state of the color correction in an object glass, particularly for the rays used in photography. Patiently and skillfully he followed up this trail, and in 1864, after many experiments in other directions, but always aiming at the same end, he succeeded

* *American Journal of Science* Vol. XXXV, p. 71.

in devising and constructing, with the aid of Mr. Fitz, an object-glass 11¼ inches in diameter and about fifteen feet focal length. This lens was corrected for photography alone and was useless for vision. A very brief account of this glass and of the prior experiments was published by Mr. Rutherfurd in the *American Journal of Science* for May, 1865.

The 11¼-inch glass was a great success and was used constantly in making negatives of Sun, Moon, and star groups. All of the superb photographs of the Moon taken before 1868 were made with this lens. Mr. Rutherfurd considered the negative made on the night of March 6th, 1865, as especially good. His photographs of the Moon were the finest ever made up to that time and have only been equalled in very recent years. The copies, which were scattered with a generous hand, attracted great attention and inspired deserved admiration. Of course Mr. Rutherfurd used wet plates, and in making moon photographs he quickly discovered that the brighter portions of the moon must have shorter exposures than the ragged edge; so he always gave his plates a skillfully graduated exposure, which is evident in the beautiful definition throughout the whole surface of his moon photographs.

In 1868 he finished his 13-inch object glass. The 11¼-inch was taken by Dr. Gould in 1870 to South America. Unfortunately it was cracked in transit. Dr. Gould put the pieces together and made some photographs with it of the southern heavens. He afterwards obtained another lens. The new 13-inch had a focal length of a little over fifteen feet. This glass was an ordinary achromatic lens and was connected with a third lens of flint glass which made the proper correction for photography, and shortened the focal length to 13 feet. This correcting lens could be fixed outside of the ordinary seeing glass, in a few minutes, by three set screws. All of the photographs taken after 1868 were made with this new instrument. Mr. Rutherfurd's photographs of the Sun were quite as remarkable as those of the moon. The series taken in 1870 showed beautifully the details of spots, the faculæ, and the mottled surface of the photosphere, and exhibited clearly the rotation of the Sun and the changes in the forms and groupings of the spots. Mr. Rutherfurd was not content with merely taking the photographs: he contrived and constructed a measuring micrometer for his plates. This was arranged to measure position-angle and distance from a central star. In the micrometer used previously to 1872, he employed screws only, but, on finding that the screw was unreliable for

long distances, be it cut ever so nicely by his own apparatus, as it needed constant investigation for errors of wear, etc., he in 1872 arranged his measuring machine with a glass *scale*, so that thereafter he depended on the screw for very small distance measures only between the divisions of the scale. This instrument will be found illustrated and described in Appleton's Cyclopædia. Doubts having been expressed in Germany as to the stability of the collodion film he published in 1872,* a series of measurements which demonstrated conclusively the fixity of the film when used upon a plate treated with dilute albumen. In 1864, Mr. Rutherfurd presented to our National Academy of Sciences a photograph of the solar spectrum obtained by using bisulphide of carbon prisms. He explained how he secured the needed uniform density of the liquid, and proved how essential this precaution was. The number of lines in the spectrum photograph was more than three times the number within the same limits on the chart of Bunsen and Kirchoff.

During 1870, Mr. Rutherfurd constructed a ruling engine described and figured in Appleton's Cyclopædia under the article "Spectrum." With this beautiful apparatus he produced superb interference gratings on glass and on speculum metal. Some of the ruled plates had 17,000 lines to the inch; they were superior to all others down to the time when Professor Rowland perfected his machine.

Mr. Rutherfurd spent a great deal of time studying the cutting operation of diamonds, and in perfecting the micrometer screw for his ruling engine. The engine was run by a miniature turbine wheel and was kept at work during the still hours of the night. Many of these ruled plates were distributed with a generous hand among the scientific men of the world.

The *American Journal of Science* for March, 1865, contains an article by Mr. Rutherfurd describing and illustrating his method for the adjustment of a battery of prisms to the position of least deviation. This method was extremely convenient. He produced a photograph of the solar spectrum with his grating (17,000 lines to the inch) which was for a long time unequalled.

Mr. Rutherfurd in 1876 gave an account of an instrument in which the divided circle was of glass. He showed that a far greater accuracy could be obtained with his glass circle than with a metallic one of the same diameter, at that time. This circle was broken during its use and Mr. Rutherfurd did not make a second one.

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* *American Journal of Science*, December, 1872.

President Grant in 1873 appointed Mr. Rutherfurd one of the scientific commission to attend the Vienna Exposition, but he was obliged to decline the honor on account of business engagements in America. In 1885 he was named by the President of the United States one of the delegates to the International Meridian Conference which met in Washington in October, 1885. He took a very active and honorable part in that conference, and was able to bring about an agreement when none seemed possible. He framed and presented the resolutions which finally expressed the conclusions of the conference. The French Academy invited him to become a member of the International Conference on Astronomical Photography held in Paris in 1887. Our National Academy of Sciences named him as its representative to the same conference. Unfortunately failing health compelled him to decline these high and merited honors. He was frequently consulted by the United States and foreign goverment officials in relation to questions of photography especially referring to eclipses of the sun, and transits of Venus. For more than twenty-five years he was a most influential member of the Board of Trustees of Columbia College, taking active part in the formation of the School of Mines and in building up the scientific work of that institution. He resigned in 1884 because he was unwilling to be absent from the monthly meetings of the Board so much of the time as his health compelled. "No man's judgment was clearer, or better informed, no man's interest keener in all that pertained to the advancement and elevation of the college, no man was a better or more judicious friend of the professors and no man's resignation as trustee could, I believe, have been more reluctantly accepted."*

Mr. Rutherfurd was one of the original members named in the Act of Congress creating ths National Academy of Sciences. For services rendered the cause of Astronomy he was made an associate of the Royal Astronomical Society of London. His work was recognized at home and abroad by many other honors conferred, such as diplomas (he was made an LL. D. at the centennial celebration of Columbia College in 1887), memberships, orders and medals; he received the Count Rumford medal.

Mr. Rutherfurd took a leading part in assisting President Barnard to form, with the aid of the late Professors Peck and Trowbridge, a department of Geodesy and Practical Astronomy at Columbia College in 1881. When the Trustees built the fine library building an Observatory was placed on the top of the

* Letter of Professor J. H. Van Amringe to the writer August 11, 1892.

edifice, and accomodations were prepared for equatorial, transit, and other instruments. In December 1883 Mr. Rutherfurd made an unconditional gift to the Observatory of his 13-inch telescope with its photographic correcting lens, his transit instrument, Dent clock, measuring micrometer, barometer and other apparatus. He was aware of the importance to science of a complete reduction of his measures on the star plates. Early in his work Dr. B. A. Gould reduced the measures made on the Pleiades and Præsepe plates taken with the 11¼-inch glass and measured with the first micrometer machine which was not provided with a *scale*.

As the scientific world is aware these results were given to the National Academy of Sciences in August, 1866, and April, 1870. In these reductions Dr. Gould showed clearly the great accuracy and value of the measures. The only publication made at the time was in the *Astronomische Nachrichten* where Dr. Gould gave the resultant distances and position angles from Alcyone for the brightest ten stars of the Pleiades group, and called attention to their close accordance with Bessel's earlier values deduced from his observations with the Königsberg heliometer. No further publication was made as Dr. Gould has explained (before the National Academy of Sciences at the New York meeting in November, 1891), because in May, 1870, he departed for South America expecting to be gone three years only, whereas his stay was prolonged to fifteen years. Moreover as Mr. Rutherfurd had in 1866 orally explained his methods to the National Academy, it was expected that he would write out a full account of his work which should antedate the full publication of the measures reduced by Dr. Gould. This Mr. Rutherfurd failed to do. Poor health and a very strong indisposition to "rush into print" (as he expressed it to me) prevented him. On Dr. Gould's return he had the original memoirs printed by the National Academy and thus twenty-two years after they were read these important communications were given to the world. During the intervening years Mr. Rutherfurd endeavored, he told me, to find some one to take up the reduction of the measures, but seemed unable to do so. His frequent and long absences from home prevented him from looking after the matter properly, and his sensitive nature would not allow him to ask anyone to attend to the work for him. It was during these years that I frequently urged him to present to Columbia College Observatory his negatives and measures, but he thought he would prefer to keep his work and arrange the reductions himself. He was exceedingly modest

about his estimate of his work on the star groups, speaking of recent improvements that had been made and saying that perhaps after all there was no demand for the reductions. Finally, however, he gave up the idea of himself directing the reductions of the measures and was persuaded that astronomers were anxious to have the work reduced. Professor E. C. Pickering's influence was weighty at this time. He offered to Columbia College $500 of the Bruce fund to publish the reductions. President Low laid this offer before Mr. Rutherfurd and he at once replied that the donation was generous but unnecessary, and that he would place in my hands the matter of the reductions and supply the needed funds.

So on November 13th, 1890, Mr. Rutherfurd gave all his negatives of sun, moon and star groups to Columbia College. With these negatives came twenty folio volumes of about two hundred pages each containing the measures of many of the plates. This valuable contribution has been placed in a fire proof vault at the College. I have given in the Annals of the New York Academy of Sciences (Vol. VI, June, 1891,) a complete list of these negatives. This catalogue shows

175 plates of the Sun	taken between		1860-74
174 " " " Solar Spectrum	"	"	1860-74
435 " " " Moon	"	"	1858-77
664 " " " Star groups	"	"	1858-77

Some of the principal star plates may be mentioned:

33 plates of 44 Bootis	taken between		1868-75
12 " " B A. C. 8083	"	"	1873-74
27 " " η Cassiopeæ	"	"	1870-73
58 " " μ "	"	"	1868-73
15 " " β Cygni	"	"	1875-76
24 " " 21 "	"	"	1875-76
22 " " 61 "	"	"	1871-76
19 " " 7 "	"	"	1875-76
27 " " Perseus Clusters	"	"	1865-74
54 " " Pleiades	"	"	1865-74
23 " " Præsepe	"	"	1865-77
23 " " 1830 Groombridge	"	"	1872-77

Many of the plates are still unmeasured.

When the collection was turned over to my care it was arranged to push forward the reductions as rapidly as possible. Mr. Jacoby of the College Observatory entered on the work at once and the results are shown in the first publication "The Rutherfurd Photographic Measures of the Group of the Pleiades." The measures reduced in this paper were made with the micrometer machine supplied with the glass scale, on plates taken with the 13-inch glass.

These reductions show conclusively that the results of Rutherfurd's measures of the Pleiades group must hereafter be taken into account with the Bessel and Elkin heliometer measures, for a study of proper motions and to form a definitive catalogue of the Pleiades. In a recent review of Mr. Jacoby's reductions Dr. Elkin states * that he shortly proposes to make a revision of the Yale Pleiades work and when that is done "the accuracy of the photographic results will be still more apparent." These results show with what ability and thoughtful care for every detail of measurement Mr. Rutherfurd directed the work of photography and of measurement; they also show that he was correct in his judgment when he stated "that the photographic method is at least equal in accuracy to that of the heliometer or filar micrometer, and far more convenient." When Mr. Rutherfurd was on his deathbed a bound copy of these reductions was placed in his hands. He was able to show his pleasure and great gratification only by the expression of his face. In the previous fall, however, he had seen most of the finished manuscript.

Mr. Jacoby has completed his reductions of the measures of the "Stars about β Cygni," and the publication will be sent out very soon. Other measures will be reduced as rapidly as possible. Rutherfurd Stuyvesant, Esq., has taken great interest in putting his father's work into available shape, and through Mr. Stuyvesant's aid the Columbla College Observatory hopes to place before the world the reductions of all the star measures. It may thereafter be desirable to measure the plates now unmeasured and to proceed to their reduction.

Dr. Gould, in an appreciative notice of Mr. Rutherfurd, has written:

"Mr. Rutherfurd was of an exceptionally amiable and generous disposition, helpful to others and tolerant of their feelings. His intellectual diffidence and almost shrinking modesty were as notable as were his boldness of invention, ingenuity of device and persistence in following up his ideas, under trying circumstances. The moral effect of his example among his co-workers, was quite as beneficent as the scientific stimulus exerted by the results he obtained and partially published. To these qualities he added a calm and unprejudiced judgment, an admirable power of statement, and every instinct of a gentlemen."†

A friend of Mr. Rutherfurd of thirty years' standing tells us in the *Photographic Times* that "The rigor of our northern winters

* *Publications of the Astronomical Society of the Pacific*, Vol. IV, No. 24.
† *Astronomical Journal*, June, 1892.

led him to spend the colder parts of the last twenty years in more southern latitudes: sometimes in the south of France but more recently amid the orange groves and tropical surroundings of Florida. While on his journey south in the autumn of last year, he contracted a severe cold, through some defect or oversight in the heating apparatus of his sleeping car, and he never fully recovered from its effects. While prostrated and weakened by this attack, the sudden death of a daughter in his northern home, produced a depression of vitality, which was lasting. In the early spring he returned to New York with his oldest son, who had passed the winter with him, and at whose residence he remained a few days. Not recovering his strength and seeming to realize that the end was near, he expressed a desire to reach his country home; the old homestead which he and his ancestors had occupied more than 150 years. Soon after reaching 'Tranquillity,' a home most appropriately named, the symptoms of failing strength became more marked, until a blood clot formed on the brain, which, although it rendered him speechless during the last few days, yet did not destroy consciousness, until the end, which came peacefully and without apparent pain" on May 30th, 1892.

COLUMBIA COLLEGE OBSERVATORY, August 25th, 1892.

DISCOVERY OF COMET BROOKS 1892.

WILLIAM R. BROOKS.

While engaged in searching the eastern heavens on the morning of August 28th, at 13 hours, I discovered a new comet, in the constellation Auriga.

The approximate position was R. A. 5 hours, 59 minutes, declination north 31° 52′. Motion was very soon detected, which was easterly. The comet was so near the path of Denning's comet, the ephemeris of which I did not have for a later date than August 5, that at first a little uncertainty was felt about its identity. But it was soon ascertained that Denning's comet passed the place of my new object a month before, and was really several degrees distant. Moreover, my comet was much the brighter of the two.

A second observation was obtained the next morning as follows: Aug. 29th, 14 hours, R. A. $6^h\ 2^m + 31°\ 48'$. This gave a daily motion of east 3 minutes south 4′.

This morning the following place was read from the circles: Sept. 2d, 15 hours R. A. $6^h\ 11^m\ 50^s + 31°\ 26'$.

The comet is an easy object in the 10-inch refractor, and a short faint tail is perceptible.

SMITH OBSERVATORY, Geneva, N. Y., Sept. 3, 1892.

Astro-Physics.

THE SPECTRUM OF COMET *a* 1892 (SWIFT).*

W. W. CAMPBELL.

I have obtained observations of the spectrum of this comet on nine mornings subsequent to and including April 5. Earlier observations with the great telescope were practically prevented by the comet's low altitude and unfavorable weather. A number of interesting changes in the spectrum were noticed, which are shown in the observed wave-lengths and in the intensity curves given below. According to Konkoly's recently published observations of April 1 and 2 (*Astr. Nach.* No. 3087) the spectrum then consisted of continuous spectrum and five bright lines; a form quite unique and different from that seen by me at any time. There are no indications in his note that any traces of the three familiar bands were seen, save that the five bright lines referred to fall within the usual limits of the bands. A decided change must have taken place between April 2 and 5. It should be noted that the comet was at perihelion April 6.

The observations were made with the spectroscope of the 36-inch equatorial. A very dense and excellent 60° flint prism by Brashear was used with magnifying power 13.3. The slit widths for the several nights are given in connection with the observations.

April 5*d* 17^h. The continuous spectrum was visible from about C to G. Its east edge was sharply defined, its west edge quite diffused. Later it was seen that these edges corresponded to the sides of the comet's nucleus towards and from the Sun, respectively. The less refrangible edges of the three characteristic bands were very sharply defined, and the middle band was terminated by a very bright line which narrowed when the slit was narrowed. The wave-lengths of these edges, corrected for the relative motion of the comet and observer were

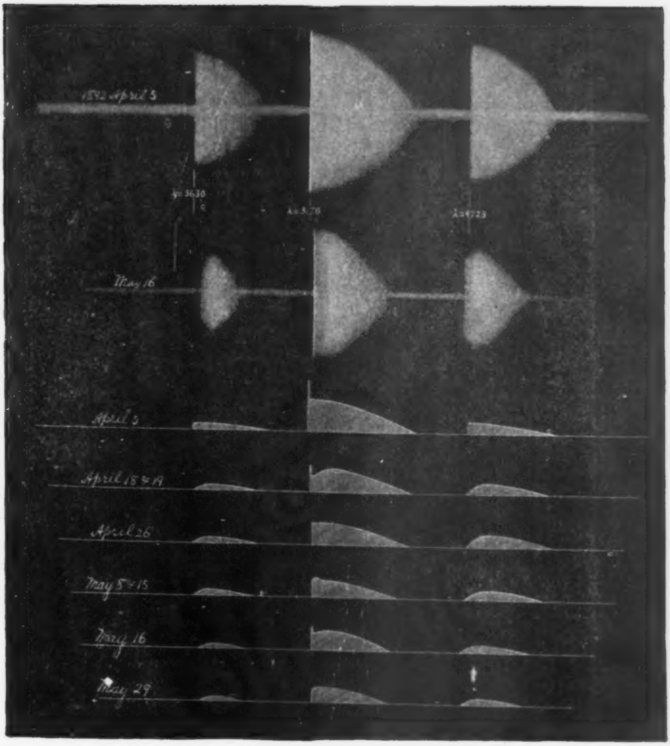
$$5630 \pm 2 \qquad 5170.3 \pm 0.3 \qquad 4723 \pm 1.$$

Slit width for first and third bands 0.006 inch; for middle band 0.003 inch. The intensities of the bands were about in the ratios 1 : 6 : 2.

April 6*d* 17^h. The comet was observed with a spectroscope attached to the 12-inch equatorial. The spectrum appeared to be the same as on the previous morning. While the spectrum was

* Communicated by the author.

PLATE XXXVI.



The Spectrum of Comet *a* 1892 (Swift).

brighter with the 12-inch than with the 36-inch, yet it could not be observed nearly so well, on account of the smaller scale.

April 18*d* 16^h. The first and third bands were not sharply terminated, and there was insufficient time to measure their wave-lengths. The wave-length of the bright line which terminated the middle band, with slit width 0.003 inch was

$$5163.1 \pm 0.4.$$

April 19*d* 16^h. The intensity curve appeared to be the same as on April 18, and the wave-length of the bright line, with slit width 0.003, was

$$5165.3 \pm 0.2.$$

April 26*d* 16^h. The bright line had disappeared. The wave-length of the less refrangible edge of the middle band, with slit width 0.003 inch, was

$$5156.7 \pm 0.4.$$

May 8*d* 15^h. Only two measures were obtained before it clouded over. The wave-length obtained with slit 0.003 inch was

$$5156.7.$$

May 15*d* 14^h. Only one measure obtained when further observations were prevented by dense fog on the object-glass. The resulting wave-length with slit 0.004 inch wide, was

$$5157.3.$$

May 16*d* 15^h. The wave-lengths of the edges of the bands were

$$5603 \pm 2.7 \qquad 5157.7 \pm 0.3 \qquad 4733 \pm 1.8.$$

The wave-lengths of the most intense parts of the bands were

$$5552, \quad 5107, \quad 4697.$$

The slit width was 0.003 inch.

May 29*d* 14^h. The wave-length of the edge of the middle band, with slit width 0.003 inch, was

$$5154.5 \pm 0.2.$$

June 13*d* 13^h. The spectrum was rendered excessively faint by fog on the object-glass and the observation is entitled to small weight. The wave-length of the middle band, with slit 0.005 inch wide, was

$$5149.3 \pm 1.4$$

These fairly accurate measures and the accompanying intensity curves make it certain that important changes occurred at the less refrangible edge of the middle band. Probably three bright lines were formed at 5170, 5164 and 5157, and disappeared in that order; so that the wave-length of the edge diminished as the comet's distance from the Sun increased. Those at 5164 and

5157 were certainly not identical; and it is improbable that those at 5170 and 5164 were identical, though the former was observed on only one night.*

By arranging the published observations of other comet spectra in the order of their dates and with reference to the times of perihelion passage, I had hoped to detect evidences of similar changes. But the large probable errors of many of the observed wave-lengths, the great differences between the results obtained by different observers, on the same night, and the entire absence of intensity curves, made it impossible to secure such evidence. In many cases the published wave-length depends upon observations made on several evenings. If it is granted that the spectrum undergoes change, it is evident that the results obtained at different times should not in general be combined, and that an accurate intensity curve is practically as valuable as an accurate measure.

ON THE SPECTRA AND PROPER MOTIONS OF STARS.†

W. H. S. MONCK.

I have more than once called the attention of the readers of ASTRONOMY AND ASTRO-PHYSICS to the greater average proper motions of Solar stars than those of the Sirian type, and I intimated my own opinion that this difference arose from the greater nearness of the Solar stars. Solar stars, in my opinion, in consequence of their small relative brightness (or intrinsic brilliancy) become invisible at distances where the corresponding Sirian stars, though not perhaps of greater mass, can be clearly detected. It was, however, possible that the greater proper motions of the Solar stars arose from their moving through space with greater velocity, and it therefore became important to ascertain as far as possible whether the spectroscope revealed any difference in their actual velocities. We have now a table of the spectroscopic velocities of 51 stars determined by Vogel which may be relied on as fairly accurate. The following is the result distinguishing Sirian from Solar stars :

* By comparing the observed wave-lengths in the vicinity of = λ 5156 with the corresponding intensity curves, it will be seen that the results are greater or less according as the edges are more or less sharply defined.

† Communicated by the author.

Sirian Stars.	Type.	Velocity. miles per second.	Solar Stars.	Type.	Velocity. miles per second.
α Andromedæ	A	+ 2.8	β Cassiopeiæ	F	+ 3.2
β Persei	A	— 1.0	α Cassiopeiæ	K	— 9.5
γ Orionis	B	+ 5.7	β Andromedæ	K ?	+ 7.0
β Tauri	A	+ 5.0	Polaris	F ?	—16.1
δ Orionis	B	+ 0.6	γ Andromedæ	K	— 8.0
ε Orionis	A	+16.5	α Arietis	K	— 9.2
ζ Orionis	A	+ 9.3	α Persei	F	— 6.4
β Aurigæ	A	—17.5	Aldebaran	K	+30.2
γ Geminorum	A	—10.3	Capella	F	+15.2
Sirius	A ?	— 9.8	Rigel	F	+10.2
Castor	A	—18.4	Procyon	F	+ 5.7
Regulus	A	— 5.7	Pollux	K ?	+ 0.7
β Ursæ Majoris	A	—18.2	γ Leonis	K	—24.0
δ Leonis	A	— 8.9	α Ursæ Majoris	K	— 7.2
β Leonis	A	— 7.6	Arcturus	K	— 4.8
γ Ursæ Majoris	A	—16.5	ε Bootis	G ?	—10.1
ε Ursæ Majoris	A	—18.8	α Serpentis	K ?	+14.0
Spica	A	— 9.2	β Herculis	K	—22.0
ζ Ursæ Majoris	A	—19.4	ε Pegasi	K	+ 5.0
η Ursæ Majoris	A	—16.3	β Ursæ Minoris	L ?	+ 8.9
β Libræ	A	— 6.0			
α Coronæ Borealis	A	+19.9			
α Ophiuchi	A	+11.9			
Vega	A	— 9.5			
Altair	A	—22.9			
α Cygni	A	— 5.0			
α Pegasi	A	+ 0.8			

Disregarding signs and taking the arithmetical mean, the average velocity of the Sirian stars is 10.8 miles per second and of the Solar stars 10.9 miles per second. Vogel gives the average (including four stars with other spectra) as 10.4 miles per second.

Admitting that this result is not conclusive, I think, when taken in conjunction with the greater surface-brightness (this term is preferable to mass-brightness) of the Sirian binary stars whose orbits have been computed, we have strong reasons for concluding that Sirian stars are on the average much more distant than Solar stars of the same magnitude, and that the reason why Sirian stars appear to be more numerous than the Solars is that they are visible at distances where the corresponding Solars are invisible with the same instruments. This conclusion may modify our opinions as to the structure of the universe. For instance, the theory sometimes adopted that the Galaxy consists chiefly of Sirian stars would be completely overthrown if we suppose that Sirian stars are on the average visible at double the distance of Solar stars.

If the stars were motionless the average velocity of 10 miles per second would mean that this was the average velocity with which the Sun approached or receded from a point in the sky

taken at random, and although the stars are no doubt moving, the result may, in this respect, prove not far from the truth. Referring the Sun's motion in space to three axes of co-ordinates at right angles to each other, we see that to give an average velocity of 10 miles per second to or from a given point, the velocity of the Sun's motion in space must be $\sqrt[2]{3} \times 10$ miles per second or between 17 and 18 miles per second. An examination of Catalogues of Proper Motion has led me to think that the Sun moves with at least this average velocity, and that consequently its speed is not likely to be less than 18 miles per second. If we knew the exact direction of its motion the corrected results of Vogel's observations would be very interesting. At present there is rather too much uncertainty for this.

THE PHOTO-ELECTRIC CELLS.*

G. M. MINCHIN.

The cells which are employed for obtaining electromotive force from the light of the stars and planets are known as *seleno-aluminium* cells. They are constructed in the following way. Take a small flat strip of aluminium about a quarter of an inch long and one-sixteenth of an inch broad; let this be heated on a clean iron plate placed over a Bunsen flame, and while it is hot let a very small bubble of melted selenium be rapidly and uniformly spread by means of a hot glass rod over about one-third of the length of the aluminium strip, the selenium forming a very thin layer. When this layer is spread, the little plate must be rapidly removed from the hot iron plate and thus cooled, while the Bunsen flame is, at the same time, removed from under the iron plate. The latter plate having become cooler, replace the aluminium strip on it, and then gradually heat up the iron plate from beneath by means of the Bunsen flame. As a result of this gradual heating, the aspect of the selenium layer on the aluminium changes; this layer changes from black to grey in appearance, and in the latter state it is sensitive to light. But to give the layer its maximum sensitiveness, several re-meltings may be necessary, until a grey surface of a somewhat brownish tinge, quite devoid of glossy streaks, is produced. Nothing but an actual sight of the process of making a sensitive plate can give the reader a correct notion of the proper kind of surface. Assuming

* Communicated by the author.

this surface produced by the gradual process of heating above referred to, the Bunsen flame is removed, and the seleno-aluminium plate is allowed to cool on the iron plate. When it has cooled (after about ten minutes) it is taken and joined to a very fine platinum wire which is inserted through a fine hole previously bored through the uncoated portion of the aluminium plate: this platinum wire is tightly pinched to the plate so as to make a good electrical contact.

So far for the sensitive plate. The cell into which it is to be inserted is a very fine glass tube about 1½ inches long, into which a platinum wire pinched to a clean plate of aluminium has been sealed: the size of this latter plate is immaterial—it may be a mere speck of the metal at the end of the platinum wire; it is the inactive plate of the cell. Into this glass tube, thus closed at one end, is inserted (by means of a pipette with a capillary stem) a quantity of pure acetone sufficient to occupy about one-quarter of the length of the tube; and then the sensitive plate is inserted until its sensitive extremity is very nearly in contact with the inactive plate, the whole of the sensitised part of the plate being covered by the acetone.

The platinum wire of the sensitive plate which now projects through the open end of the cell must be sealed into the tube, the end of the tube being, of course, completely closed by the sealing. Much practice is here necessary to prevent the vapor of the acetone from bursting the heated end of the tube; but the process becomes easy enough with practice.

The cell is now made, and if its poles are connected with those of an electrometer, and light is allowed to fall on the sensitive plate, an electromotive force will be indicated.

Shortly after the cell has been made, it is wonderfully quick in its response to changes of the incident light—almost instantaneous, in fact; but after about 24 hours, it becomes slower in its response. The cause of this is not yet quite known; but it has been found that a constant régime can be produced and kept up for months by—

(*a*) using perfectly pure acetone,

(*b*) using perfectly pure selenium,

(*c*) turning the cell upside down when it is not required for use, and thoroughly shaking the liquid away from the plates.

The complete and permanent elimination of sluggishness from the cell is under consideration at present.

As regards the ***magnitude*** of the electromotive forces produced, it may be said that ordinary diffused daylight falling on the sen-

sitive plate will give an E. M. F. of about ½ volt, which is surprisingly great. A candle at a distance of 7 feet will give about $\frac{1}{30}$ volt.

Light of all refrangibilities from red to violet is effective—and this fact distinguishes this cell from every other known photo-electric cell—the maximum effect being produced by the yellow rays; but there is not very much difference between the effects of the various parts of the spectrum.

By putting a number of these cells in series, the effect is multiplied by the number employed; thus 10 cells in series will give 10 times the E. M. F. of one cell.

Hence for stellar observations the cells should be made as small as possible, and cells much smaller than the typical one above described have been made.

Does anything depend on the *size* of the sensitive plate? It would appear that nothing depends on the size, and that therefore a mere pin point of sensitive surface is as effective as a square centimètre. Perhaps this is so; but it has been found that the maximum E. M. F. is never given when the sensitive surface is as small as a large pin head. For stellar observations this is most unfortunate; but it is highly probable that the result is due to the large size and capacity of the electrometers at present at our disposal. There is good reason to think that, with an extremely small electrometer, the pin-head plates will give as good results as the larger ones. Certainly with a common quadrant electrometer a sensitive surface 6 millimètres long and 2 milemètres wide gives as good a result as a surface 10 times as large. For the light of the Moon there is no difficulty in making batteries of photo-cells containing 10 or 20 cells.

With Mr. Monck's refracting telescope, the image of Mars would take, perhaps, *three* cells, and an unmistakable E. M. F. should be produced. Jupiter would take more; but it would be difficult to cover *completely* the sensitive surfaces of *two* cells with the light of Vega. (The *whole* of the sensitive surface of every kind of photo-cell must be covered by the incident light to obtain the full effect).

The best existing form of electrometer is Clifton's form of Thomson's Quadrant. Some very slight improvements in this instrument would render it fairly fit for photo-electric observations in an Observatory. When working well (well insulated, and preserved from draughts of air) it will give about 200 half millimèters deflection on a scale distant 1 mètre from the mirror for 1 volt. Hence a candle at 7 feet from one photo-cell has been

found to give about 7 divisions deflection. Thus it is very easy to get results from moonlight; and, with a clear sky and the absence of air currents, the light of a planet should be easily measurable.

For a given source of light, the E. M. F. developed in a photo-cell varies inversely as the distance of the light from the cell.

Instead of an electrometer, a high resistance reflecting galvanometer could be used with photo-cells; but the former instrument is far preferable, because it is not advisable to allow currents to circulate in the cell. A galvanometer and a condenser (the latter charged by the cell while light falls on it, and then suddenly discharged through the galvanometer) give enormous deflections with moonlight; but this method is objectionable.

So far as is known at present, these cells will stand any amount of exposure to light without deterioration—provided that they are always employed with an electrometer, *i. e.*, open-circuited.

Mr. Monck and Professor Dixon have, I believe, succeeded in obtaining results from the light of Mars under most unfavorable atmospheric conditions. I remained in Dublin for a week in the beginning of August to try the cells with the stars; but during this time not a single opportunity occurred, the sky being heavily clouded every night.

When a photo-battery has been used with a strong light, such as that of the Moon, the deflection on the electrometer scale takes some time to disappear when the light has been shut off. This deflection can, however, be very quickly got rid of without injury to the battery by an instantaneous connection of the battery with a Daniell cell whose zinc pole is for the moment connected with the sensitive pole of the battery, the copper being connected with the insensitive pole and with earth.

ROYAL ENGINEERING COLLEGE,
Cooper's Hill, England.

ON THE SPECTRUM OF LIQUID OXYGEN, AND ON THE REFRACTIVE INDICES OF LIQUID OXYGEN, NITROUS OXIDE, AND ETHYLENE.*

PROFESSORS LIVEING AND DEWAR.

In September, 1888, were described in this Magazine (p. 286) the absorption-spectrum of oxygen gas in various states of com-

* From the *Philosophical Magazine* for August, 1892.

pression. At lower pressures the absorptions known in the solar spectrum as A and B were most conspicuous, and as the pressure increased the other bands described by Janssen came out with increasing intensity. The former appear to be due to the molecules of oxygen, and increase in intensity directly with the mass of the oxygen producing them; while the latter appear to arise from the mutual action of the molecules on one another, since their intensity is dependent on the density as well as the mass of the oxygen producing them.

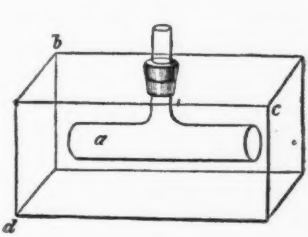
With the small dispersion employed in these observations the absorptions A and B were not resolved into lines as in the solar spectrum, but they had otherwise the same general characters: A consisted of two bands, and both A and B were sharply defined on the more refrangible edge and gradually faded out on the less refrangible side. Considering how much more diffuse the lines forming these groups in the solar spectrum become as the Sun gets nearer the horizon (see McClean's photographs), it is probable that, under the circumstances of our experiments, they would not have been resolvable into lines even with higher dispersion.

Subsequently, in a paper read at the Royal Society (*Proc. Roy. Soc.*, vol. XLVI, p. 222), we described our observations on the absorption of a thickness of 12 millim. of liquid oxygen. We noticed, as Olszewski had done, the strongest three of the diffuse bands seen in the spectrum of the compressed gas, but could not detect A. The mass of oxygen in 12 millim. of the liquid was not enough to make A visible.

We have since made observations with larger quantities of liquid oxygen. For this purpose we have used a glass tube of the form shown at *a* in the annexed figure, about $\frac{3}{4}$ inch in diameter and 3 inches in length. This tube had the ends blown as flat and clear as possible, and it was enclosed in a box with glass sides *b c d*, and the air in the box well dried, in order to prevent the deposition of hoar-frost on the tube. The liquid oxygen was poured into the tube at the pressure of the atmosphere, and at first, of course, boiled violently, until the tube was reduced to the temperature of boiling oxygen, —181°, after which the liquid boiled slowly and quietly. Through the length of the tube (that is, a thickness of about 3 inches of liquid oxygen) we viewed the hot pole of an electric arc with a spectroscope having two calcite prisms of 30° and one of 60°. As reference-rays we used the red potassium-lines, of which the positions with reference to A and B were well determined by Kirchhoff, and confirmed by our own

observations. These lines were easily obtained by dropping a little of a potassium salt into the arc.

The diffuse bands previously seen both in the gas and liquid were all of exceptional strength, but we did not notice any addition to their number except a faint band just above G. In place of A we observed a band, but different from A in the following remarkable particulars. Instead of having a sharp edge on the more refrangible side and fading gradually towards the less refrangible side, its position appeared to be reversed; the sharp edge was on the less refrangible side, and it faded away gradually on the more refrangible side. Moreover its sharp, less refrangible edge did not coincide with the sharp edge of A, but reached very nearly to the more refrangible of the two potassium-lines, that is, had a wave-length of nearly 7660. At the same time the band extended beyond the sharp edge of A on the more refrangible side. There was no indication that it was resolvable into lines, or even into two bands. Turning to the place of B in the spectrum we were not able, with that thickness of oxygen, to detect any band in that place. Olszewski (Wied. *Ann.* XLII, p. 663), with a thickness of 30 millim. of liquid oxygen, observed a somewhat faint band corresponding to A, which with a Rutherford prism was not resolvable into lines, but he has not noticed the reversed position of the band.



Using a similar tube for the liquid oxygen, but six inches long, the band at A came out very much stronger and extended much further on the more refrangible diffuse side, but was not conspicuously expanded on the other side, and did not hide the potassium-line. At the same time a fainter band appeared at the place of B. This had precisely the same character as that of A; that is, it had its sharp edge on the less refrangible side and faded gradually on the more refrangible side. Its sharp edge also did not coincide with the sharp edge of B, but reached nearly to the red potassium-line λ 6913. By estimation, using the potassium-lines for comparison, we put the wave-length of the less refrangible edge at about λ 6905, while its diffuse side was visible to about λ 6870, that is, barely to the place of the strong edge of B.

It is plain that these two bands are related to each other in the

same way as the solar groups A and B are related, and we cannot avoid the conclusion that they represent A and B, but modified by the change of the absorbent from the gaseous to the liquid state.

If, as there is good reason to think, A and B are the absorptions of free molecules of oxygen, the persistence of these absorptions in the liquid seems to show that the molecules in the liquid are the same as in the gas. At the same time the changes they undergo ought to throw some light on the nature of the change in passing from the gaseous to the liquid state, as well as on the causes which produce the sequences of rays which are called channelled spectra.

We have noticed, as Olszewski also has noticed, that liquid oxygen is distinctly blue. This is, of course, directly connected with its strong absorptions in the orange and yellow. On looking at a mass of liquid oxygen through a direct-vision spectroscope in any direction the scattered light shows the strong bands plainly. Indeed they remain visible when the oxygen has evaporated to the last drop, and they increase in intensity as the liquid is cooled, so that when the pressure on the liquid is reduced and the oxygen cooled by its own evaporation to —200° they become exceedingly black. Olszewski states that this blue color is not, so far as he could make out, due to ozone, and we are of the same opinion. Ozone dissolves easily in liquid oxygen and imparts to it an indigo-blue color. Such a solution when poured into a saucer of rock-salt assumes the spheroidal state, and as the oxygen evaporates becomes more concentrated, and finally explodes with considerable violence. In the dilute solution we could not detect any absorptions due to the ozone. We attempted to obtain a larger quantity of liquid ozone, or of a concentrated solution, for the observation of its spectrum. Oxygen ozonized in a tube cooled by solid carbonic acid gave small beautiful cobalt-blue drops of liquid, but when a few of these drops collected together in a tube immersed in liquid oxygen to cool it to — 181°, they exploded and blew the whole apparatus to pieces, comminuting the tube to fine powder. This instability of ozone, equally at very low and at high temperatures, is a significant fact in regard to the form of chemical energy. It seems probable that it is connected with the great absorbent power of ozone. The radiant energy absorbed must give rise to molecular movements which may, we conceive, set up disintegration.

The determination of the refractive index of liquid oxygen, at its boiling point of — 182° C., presented more difficulty than

would have been anticipated. The necessity for enclosing the vessel containing the liquid in an outer case to prevent the deposit of a layer of hoar-frost which would scatter all the rays falling on it, rendered manipulation difficult; and hollow prisms with cemented sides cracked with the extreme cold. It was only after repeated attempts, involving the expenditure of a whole litre of liquid oxygen on each experiment, that we succeeded in getting an approximate measure of the refractive index for the D line of sodium. The mean of several observations gave the minimum deviation with a prism of 59° 15′ to be 15° 11′ 30″, and thence $\mu = 1.2236$, The density of liquid oxygen at its boiling point of — 182° C. is 1.124, and this gives for the refraction-constant, $\frac{\mu - 1}{d} = 1.989$, and for the refraction-equivalent 3.182. This corresponds closely with the refraction-equivalent deduced by Landolt from the refractive indices of a number of organic compounds. Also it differs little from the refraction-equivalent for gaseous oxygen, which is 3.0316. This is quite consistent with the supposition that the molecules of oxygen in the liquid state are the same as in the gaseous.

If we take the formula $\frac{\mu^2 - 1}{(\mu^2 + 2)\,d}$ for the refraction-constant we find the value of it for liquid oxygen to be .1265, and the corresponding refraction-equivalent 2.024. These are exactly the means of the values found by Mascart and Lorenz for gaseous oxygen. The inherent difficulties of manipulation, and the fact that the sides of the hollow prism invariably became coated with a solid deposit, perhaps solid nitrogen, which obscured the image of the source of light, have hitherto prevented our determining the refractive indices for rays other than D.*

The determination of the refractive indices for liquid nitrous oxide did not present so great difficulties. The minimum deviations for the rays C, D, F, G, and for the lithium ray λ 6705.5, and the indium ray λ 4509.6, were found to be, respectively, 22° 53′, 23°, 23° 18′, 23° 33′, 22° 52′, and 23° 28′. The corresponding values for μ are 1.329, 1.3305, 1.3345, 1.3378, 1.3257 and 1.3368.

* This will be prosecuted further, however. The refractive index of oxygen has an important bearing on the electro-magnetic theory of light, considering that we are dealing with a magnetic liquid. The polarizing angle corresponding to the index of refraction found above for liquid oxygen is 50° 45′, and one of us has found that when liquid oxygen is cooled to — 200° by its own evaporation at reduced pressure so as to present a steady surface, and the image of a candle is viewed by reflection at that surface the light is very completely polarized when the incidence is at that angle.

The specific gravity of liquid nitrous oxide at its boiling point of $-90°$ C. was found, by weighing 100 cubic centim. of the liquid, to be 1.255.

This gives, for the D ray, $\frac{\mu - 1}{d} = 0.2634$ and for the molecular refraction 11.587. Or, if we take the other formula, $\frac{\mu^2 - 1}{(\mu^2 + 2)d} = .163$ and the corresponding molecular refraction 7.163. Subtracting the refraction-equivalent for oxygen we get for the molecular refraction of nitrogen 8.405 or 5.139 according to the formula used. Mascart's determination of the index of refraction of gaseous nitrous oxide for the D ray was 1.000516 and the corresponding molecular refraction 11.531, or 7.69, according to the formula used, and in this case the older formula for the refraction-equivalent satisfies the condition of continuity between the gaseous and liquid states better than the newer.

It was more difficult to obtain the refractive indices for liquid ethylene on account of its irregular boiling. Liquid oxygen and nitrous oxide boil steadily, but ethylene in sudden bursts of large volumes of vapor. The minimum deviation for the D ray was found to be 25° 29′, approximately. This gives $\mu = 1.3632$, and, since the density of the liquid at its boiling-point of $-100°$ C. is 0.58, $\frac{\mu - 1}{d} = 0.627$ and $\frac{\mu^2 - 1}{(\mu^2 + 2)d} = 0.384$. The corresponding numbers for gaseous ethylene, according to Mascart, are 0.578 and 0.385. The agreement for the second formula is close, but we doubt if much stress can be laid on this, inasmuch as we know that the liquid ethylene contained a small quantity of ether.

RESUME OF SOLAR OBSERVATIONS MADE DURING THE FIRST QUARTER OF 1892.*

P. TACCHINI.

The number of days of observation has been 82, *i. e.*, 21 in April, 31 in May and 30 in June. The following are the results:

1892.	Relative Frequency of spots.	of days without spots.	Relative Size of spots.	of faculæ.	Number of groups per day.
April	24.67	0.00	70.81	51.19	5.57
May	24.27	0.00	119.47	62.50	5.74
June	25.0	0.00	111.20	106.83	6.20

We thus find an augmentation in the phenomena of Sun-spots, and also in the faculæ.

* Communicated by the author.

For the prominences we have obtained the following results:

1892.	No. of days of observation.	Prominences Mean Number.	Mean Height. ″	Mean Extent. °
April	19	7.84	38.7	2.0
May	27	7.70	38.2	1.9
June	30	10.63	37.5	1.7

The prominences have been more numerous than in the preceding quarter, and this agrees with the spots, for the secondary maximum occurred also in the same month of June. We have thus entered upon the truly maximum period of the solar activity.

R. OSSERVATORIO DEL COLLEGIO ROMANO,
Rome, 15 July, 1892.

DISTRIBUTION IN LATITUDE OF SOLAR PHENOMENA OBSERVED DURING THE SECOND QUARTER OF 1892.*

P. TACCHINI.

The following results were determined for each zone of 10°, in both hemispheres of the Sun:

1892.	Prominences.		Faculae.		Spots.		Eruptions	
90° + 80°	0.000							
80 + 70	0.013							
70 + 60	0.106							
60 + 50	0.065							
50 + 40	0.053	0.471	0.004					
40 + 30	0.073		0.033		0.011			
30 + 20	0.084		0.111	0.473	0.085	0.512		
20 + 10	0.039		0.202		0.308		0.667	0.667
10 . 0	0.038		0.123		0.106		0.000	
0 − 10	0.033		0.074		0.000		0.000	
− 10 − 20	0.062		0.156		0.234		0.111	
− 20 − 30	0.085		0.206	0.527	0.202	0.490	0.111	0.333
− 30 − 40	0.106		0.091		0.054		0.111	
− 40 − 50	0.091	0.529	0.000					
− 50 − 60	0.115							
− 60 − 70	0.037							
− 70 − 80	0.000							
− 80 − 90	0.000							

The prominences and faculæ have been a little more frequent in the southern hemisphere, while the spots and eruptions show a maximum in the same zone (+ 10° + 20°) north of the equator. The maximum for prominences occurs farther from the equator than was the case during the preceding quarter, but prominences

*** Communicated by the author.**

are still lacking in the vicinity of the poles. In examining Professor Hale's beautiful photographs of faculae on the solar disc I foresee that he will arrive at the same conclusion that has resulted from my own observations, *i. e.*, that the phenomena which are in closest accord with the prominences are the faculæ, while spots and eruptions are always confined to low latitudes.

R. OSSERVATORIO DEL COLLEGIO ROMANO,
Rome, 27 August, 1892.

NEW RESULTS ON HYDROGEN, OBTAINED BY SPECTROSCOPIC STUDY OF THE SUN.—COMPARISON WITH THE NEW STAR IN AURIGA.*

H. DESLANDRES.

The complete spectrum of hydrogen was observed for the first time in white stars by Dr. Huggins, who succeeded in adding ten new ultra-violet lines to the four lines previously known in the visible region. This result was afterwards confirmed by Lockyer, Vogel and Cornu, who found in laboratory experiments on incandescent hydrogen successively one, four and nine of these new lines.

A short time later M. Balmer pointed out a simple function of successive whole numbers which exactly represents this series of fourteen lines, which is comparable to a series of harmonic tones. This remarkable function, which also applies to the greater part of the metals, is the following:

$$N = A - \frac{B}{n^2};$$

N being the number of vibrations, A and B two constants, and n a whole number varying between 3 and 16.

The series of harmonics of hydrogen, which in the state of dark lines characterizes the white stars, can with difficulty be obtained only as a faint and incomplete series in the laboratory. But I have recently obtained it in the Sun, brilliant, very intense, unbroken, and with five additional new lines, under such conditions as allow the precise measure of the vibration numbers.

These upper harmonics of hydrogen do not show themselves, as is well known, in the disc of the Sun, which is a yellow star, but they appear distinctly in the most brilliant parts of its atmosphere, as I have already pointed out (see *Comptes rendus*, Aug. 1891, Feb. and March, 1892). On the fourth of May last I pho-

* Comptes rendus (Paris), 25 July, 1892.

tographed* the region of the spectrum between λ 400 and λ 360 of a remarkably brilliant prominence, which was characterized by the richest and most complete radiation so far observed in this region. In fact, the negative which I have the honor to present to the Academy, exhibits, in addition to a large number of metallic lines enumerated at the bottom of the page,† the ten ultra-violet hydrogen lines of Dr. Huggins, and five new lines in addition, which follow the preceding ones with such regularity that one is led to assign these also to hydrogen. Moreover these bright lines are projected on the spectrum of the diffuse light of the sky, or of the Sun, which is at present more exactly known than any other spectrum. I have thus been able to measure with precision their vibration numbers as referred to Professor Rowland's fundamental lines. The table below allows a comparison of the vibration numbers thus measured by me, the vibration numbers obtained in the laboratory by Mr. Ames, and the numbers calculated by Balmer's formula with the constant determined by Ames on the visible lines:‡

$$N = 274.1831 - \left(\frac{4}{n^2}\right).$$

	Whole Numbers in the formula. n	Huggins' notation.	Vibration numbers obtained by me.	obtained by Ames.	calculated.
Observed in the Stars	12	Hε	266.565	266 575	266.566
	13	Hζ	267.685	267.715	267.694
	14	Hη	268.585	268.615	268.586
	15	Hθ	269.310	269.330	269.309
	16	Hι	269.890		269.898
Observed in the Sun.	17		270.385		270 387
	18		270.795		270.797
	19		271.140		271.142
	20		271.460		271.448
	21		271.700		271.694

It is seen, on the one hand, that, for the lines already known, the difference between the observed and calculated values is less for our measures than for those of Mr. Ames, our measures having been made under more favorable conditions; and on the other hand, that the new lines correspond exactly with the five succeeding terms of Balmer's formula. Consequently these lines

* This photograph was obtained with the aid of my assistant, M. Mittau.

† The principal lines (not corrected for refraction) are: λ 396.66, λ 394.41 of aluminium; λ 383.84, λ 383.25, λ 382.95 of magnesium, which are reversed; λ 385.65, λ 382.05, λ 381.60, λ 374.84, λ 374.58, λ 373.73, λ 372.01, λ 370,59 of iron; and also the lines λ 392.81, λ 392.30, λ 390.56, λ 388.64, λ 382.80, λ 382.60, λ 382.46, λ 381.98, λ 376.14, λ 375.93, λ 368.35, λ 368.52 which have not yet been assigned to any known element.

‡ These vibration numbers are corrected for atmospheric refraction.

belong to hydrogen, and we see it once more verified that this very remarkable formula represents the hydrogen vibrations better as the observations gain in extent and precision.

Our theoretical knowledge of hydrogen, which has been increased by the study of the stars, is thus completed by the study of the Sun, which is, it is true, the most intense source of light we can employ.

COMPARISON WITH THE TEMPORARY STAR IN AURIGA.

But this exceptional prominence is still of further interest on account of the comparison it allows with the temporary star in Auriga. In fact, the spectrum of this star, in the region included by the photograph, is identical in composition with that of the prominence; and this result strongly supports the explanation given by Dr. Huggins, who attributes the temporary brilliancy of the star to enormous prominences produced by the approach of two neighboring bodies.

The spectrum of the star is formed of lines grouped in pairs, a bright line being accompanied by a dark line, both bright and dark lines showing reversals, with a constant displacement of the reversed lines. Now the bright lines of calcium at the base of the prominence are also reversed. Moreover, when the prominences, instead of being on the limb, are projected on the disc of the Sun, appearing then among the faculæ, the dark lines of calcium always show a very distinct double reversal*, similar to that of the new star.

But the similarity is still more striking when we examine, no longer a particular point on the Sun, but the whole of the Sun, as in the case of the stars, by allowing light from all points to pass into the apparatus simultaneously; the double reversal of the faculæ is still present,—†although less intense—if the Sun is rich in faculæ; it is proportional to their brilliancy and extent. Moreover, when the faculæ, which are considered as grouped in the same region, are approaching or receding from the Earth on account of the solar rotation, the reversed lines are displaced with reference to the whole spectrum. Thus, and this fact is worth pointing out, the Sun sometimes exhibits one of the most singular phenomena of the new star.

These bright lines of reversal represent the whole of the elevated incandescent gaseous masses of the atmosphere, and their

* I was the first to point out this property of the faculæ, which Mr. Hale has confirmed in parallel investigations.

† It may be masked by prominences on the limb, when they are very brilliant. It may be obtained when the Sun is invisible, with the light of the clouds.

displacements with reference to the other lines are due to the rotation of the star. As they are found in the Sun it is natural to look for them in the stars; and certainly, with the large telescopes at present employed, the brightest stars can be analyzed almost as well as the Sun. The study of these reversals will furnish valuable data on the nature and rotation of the atmospheres of stars, and will permit problems to be attacked, which have up to the present seemed quite beyond our reach.

OBSERVATOIRE DE PARIS.

RECENT OBSERVATIONS OF NOVA AURIGÆ.*

W. W. CAMPBELL.

The new star in Auriga was clearly seen with the 36-inch telescope on April 24, when it was of the sixteenth magnitude or fainter. It was occasionally glimpsed late in the evening of April 26, when its altitude was small. Further observations were prevented by a three weeks' storm, at the close of which the star was too low in the west to be observed. The rapid decline in brightness made it probable that it would soon disappear from sight. But it was again observed by Professors Holden and Schaeberle and myself on August 17, when its magnitude was estimated at 10.5. All the observers agreed that its appearance was different from that of other stars of the same magnitude, in that its disk was larger and its light duller. However, the moon was only a few degrees east of the star and the bright sky interfered with further observations on that point. A direct vision spectroscope of very small dispersion showed its spectrum to consist of three bright lines and a faint continuous spectrum. The instrument did not permit of measures being made to determine the wave-lengths, and the telescope was not available again for spectroscopy for several days.

On August 19 (15 hours), with a more powerful spectroscope attached to the 12-inch telescope, the brightest line previously observed was resolved into three lines. These were at once recognized to be the three characteristic nebular lines, and thus the nebulous character of the object was established. By bringing the lines into contact with a bar in the focus of the eyepiece and turning to β Tauri and Venus the wave-lengths were estimated to be 501, 496 and 486. The faint continuous spectrum

* Communicated by the author.

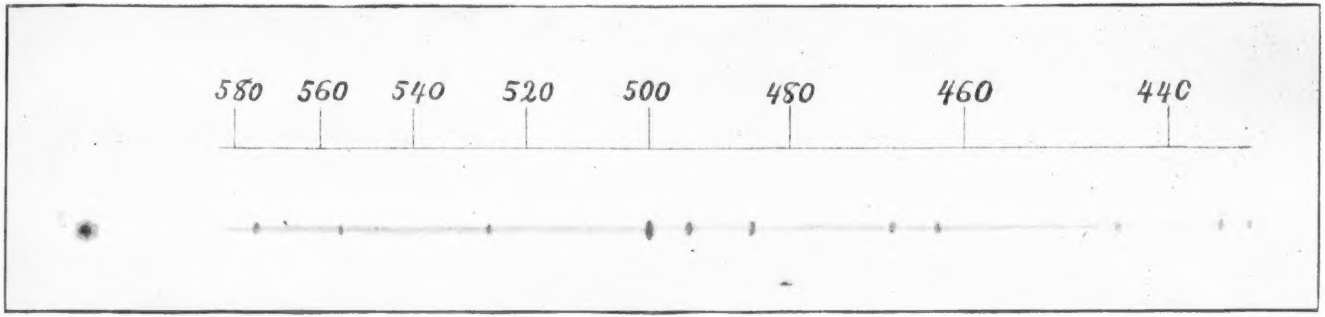
was just visible. The magnitude of Nova was noted as midway between that of Pickering's comparison star *s* and the $9^m.5$ star DM. + 30°.920: that is, about $9^m.9$. Mr. Townley estimated it at $0^m.2$ brighter. No appreciable change in brightness has yet been observed.

The same morning Professor Barnard, using the 36-inch telescope, observed the Nova as a nebula 3″ in diameter, with a tenth magnitude star in its centre.

Further study of the spectrum with the large spectroscope has shown eleven bright lines and a continuous spectrum corresponding to a star of the eleventh magnitude or fainter. The positions of nine of these lines have been quite accurately determined; one at λ 5268 was measured only once; and another estimated at λ 557 could not be measured with the large spectroscope, though it was twice clearly seen with a small spectroscope using weak dispersion. Two others in the green and a line near C were suspected on different occasions, but they could not be located surely. The continuous spectrum presents the appearance of containing a large number of bright lines, just beyond the power of the telescope to define.

Below is a table of the wave-lengths of the lines. They are reduced to the Sun. The difficulty with which the several lines were measured permitted the relative intensities to be estimated very accurately. The lines at λ 4466 and λ 4336 are not visible to me, and their intensities were estimated from the photograph by comparison with the line λ 4360. The unmarked wave-lengths were obtained with the dense 60° flint prism and the 10½-inch observing telescope, using a magnifying power of 13.3. In obtaining those marked with an asterisk (*) the prism was replaced by a second order grating of 14,438 lines to the inch. In obtaining those marked thus (†) a first order grating was used. The one marked thus (‡) was obtained with a thallium compound prism. Those marked thus (§) were obtained photographically, using the 60° prism and replacing the micrometer by a camera.

PLATE XXXVII.



Spectrum of Nova Aurigæ.

Intensity.	Aug. 20	Aug 21	Aug. 22	Aug. 23	Aug. 30	Sept. 3	Sept. 4	Sept. 6	Sept. 7
I	5746	5751	5750	5752	......	......	......	......	......
0.2	......	......	......	......	[557]	......	......	......	......
0.3	......	5268	......	......	......	......	......	......	......
10	5003.6	5003.7	5003.7	*5003.1	5002.9	5002.4	†5001.97	§5001.9	†5001.83
.....:	......	......	......	......	‡5002.3	......	*5001.80	§5002.4	*5002.00
......	......	......	......	......	*5002.24	......	§5001.9	......	§5002
3	4954	4953	4954	......	......	......	§4953.3	§4953.3	§4953
......	......	......	......	......	......	......	......	§4952.1	......
I	486	4858.4	4858.3	......	4857.3	4856.8	§4856.7	§4856.7	......
0.4	4677	4684	4685	......	......	......	......	§4678	4680
......	......	......	......	......	......	......	......	......	§4685
0.7	4628	4633	4631	......	......	......	......	......	4628
......	......	......	......	......	......	......	......	......	§4634
0.1	......	......	......	......	......	......	......	......	§4466
0.8	436	4357.1	4358.9	4358.0	......	4358.9	......	......	§4360
0.1	......	......	......	......	......	......	......	......	§4335.9

[In the photograph of Sept. 7, the lines between λ 5002 and λ 4466 are slightly out of focus, and the results are of small weight.]

The spectrum resembles that of the planetary nebulæ. The lines at λ 5002, λ 4953 and λ 4857 are undoubtedly the three nebular lines, displaced toward the violet about five tenth-metres.* The nebula is therefore approaching us with a velocity of at least 175 miles per second. The presence of a prominent line at λ 4358 and the fact that no line is visible in the position of Hγ led me to infer that the Hγ line is absent. But a photograph of the Hγ region obtained this morning with a two hours' exposure shows not only the very bright line λ 4360 but also the bright Hγ line at λ 4335.9 and a trace of a line at λ 4466. The displacement of the Hγ line is thus shown to be similar and equal to that of the three nebular lines. The line λ 4466 undoubtedly corresponds to the line λ 4471 in the planetary nebulæ Σ 6, and shows the proper displacement. The line λ 4681 corresponds to the line λ 4685 in some of the planetary nebulæ and is properly displaced. Its positions in the spectra of N. G. C. 7027 and 7662 were measured last night and found to be λ 4685 and λ 4684 respectively. The lines λ 5268 and λ [557] are possibly the lines λ 527 and λ 554 seen in the Orion Nebula and some of the planetary nebulæ. So far as I know nebular lines have never been observed in the positions λ 5755, λ 4635 and λ 4364, though the line λ 463-4 is present in some of the bright line stars. Possibly a careful examination of the planetary nebulæ would reveal some or all of them. There is no visible trace of a line in the D_3 region.

* Professor Keeler's adopted wave-length of the first nebular line is 5007.05.

The line λ 5002 has been more refrangible than the lead line λ 5005.63 on every night. But on Aug. 30 the distance between them seemed greater than usual. To confirm this point the difference of wave-length was measured successively with the 60° prism, the compound prism and the second order grating. The displacement of the line λ 4858, which had just previously been measured, shows a similar and equal variation. If the variation is real and progressive, the later measures do not confirm it as strongly as could be wished. But the perfectly independent measures on the two lines most carefully compared leave little doubt that some change has occurred. The difficulties in the way of deciding the question arise not from the faintness of the lines, but from their great breadth. They are more diffuse than those of any of the planetary nebulæ which I have observed. With the grating the line λ 5002 is at least eight tenth-metres broad, with diffuse edges, and a brighter central region about four tenth-metres broad. On Aug. 30 the line was suspected to be double, and the grating measures of that night refer to a point midway between the two condensations. On Sept. 7 the measures refer to a point of maximum brightness slightly less refrangible than the centre of the line. The other lines are too faint to be observed with the grating and narrow slit.

In order to test the adjustments of the spectroscope, the positions of the lines in the planetary nebula Σ 6 were measured on several nights, using the 60° prism. The three nebular lines and the Hγ line occupied their normal positions, within the errors of observation. On Aug. 23 the position of the first nebular line in Σ 6 was measured with the second order grating and found to be λ 5006.93 (corresponding to a velocity of approach of 4.5 miles per second). The mean of Professor Keeler's observations on thirteen nights is λ 5006.89 (corresponding to a velocity of approach of 6.0 ± 1.2 miles). Turning to Nova without changing any of the adjustments the wave-length of the corresponding line was measured and found to be 5003.1 Similarly, on Sept. 4, the position of the first nebular line in nebula N. G. C. 7027 was found to be 5007.18 (corresponding to a velocity of recession of 4.8 miles per second). From four nights' observations Professor Keeler obtained a velocity of recession of 6.3 ± 1.2 miles. Turning to Nova, the wave-length of the first nebular line was found to be 5001.9.

The relation of this spectrum to the early one of February and March is not apparent. The lines λ 4858 and λ 4336 coincide almost exactly with the sharp edges of the original bright F and

Hγ lines. If we also except the line λ 4630, then the remaining lines fall upon comparatively thin (dark) regions of the original spectrum. All of the lines now present, however, could have been present in the early spectrum when it was observed here and have escaped detection. The brighter continuous spectrum would mask them effectually in visual observations and also in the photographic, even if the exposures had been long enough to record them. The line λ 4630 falls upon a broad bright line of the original spectrum.

Visual observations on three nights in February and traces of a line on two photographs fix the position of a faint line at λ 4969. At first there seemed very little reason for suspecting it to be the second nebular line. But after reducing the photographic observations, and considering it in connection with a faint line at λ 5885 (D_3?) observed March 13, with all or a portion of the very bright line λ 5016, and with the less refrangible components of F and Hγ shown on the photographs at λ 4871 and λ 4348, there was seen to be a possibility that these were nebular lines, displaced about ten tenth-metres toward the red. It is important to note that in the original spectrum there were also prominent lines at λ 5577, λ 5281, λ 4481, and later at λ 5761: and the *only* prominent lines in those regions. The *relative* positions of these lines agree remarkably well with those of the present lines (excepting the lines λ 4630 and λ 4360). But the two sets of lines could be related to each other only by an enormous change in the velocity of the light source (about 500 miles per second). On the hypothesis of two bodies and a period of several months, this would be possible only with a very excentric orbit, the major axis and periastron probably directed towards the solar system, and the proper relation existing between the masses of the two bodies. The evidences of increasing velocity of approach in the recent measures support the hypothesis. But the evidence of fairly *constant* velocity during February and March is opposed to it. If the hypothesis is tenable future observations will show increased velocities. If further observations show no increase of velocity the hypothesis is probably untenable. As stated above, it apparently does not explain the presence of the lines λ 4630 and λ 4360.

Mr. S. D. Townley, Fellow in Astronomy, kindly assisted in making and reducing the observations.

MT. HAMILTON, 1892, Sept. 8.

OBSERVATIONS ON THE THERMAL ABSORPTION IN THE SOLAR ATMOSPHERE, MADE AT POTSDAM.*

EDWIN B. FROST.

I. OBSERVATIONS ON THE PHOTOSPHERE.

The absorptive properties of the solar atmosphere have received the attention of several observers since Bouguer's original attempt to measure the different intensity of the light at different distances from the center of the Sun's disk. The absorption of the "photographic" rays of short wave-length was carefully investigated by Vogel in 1872 (Berichte der K. Sächs. Ges. d. Wiss. 1872 Juli).

Secchi, Liais and Pickering made extended series of photometric observations in the years 1852-74 on the diminution of intensity for the rays of medium wave-length from the center toward the edge of the disk, but the most complete investigation was made by Vogel† in 1876 with the use of his modification of the Glan spectral-photometer, whereby exact values of the absorption were obtained for six different portions of the Spectrum between $\lambda = 405$ and 658 $\mu\mu$.

By means of the thermopile the absorption for the heat rays has been studied by Secchi, Langley, Cruls and others. The results are subject to very considerable discordances. As Professor Vogel has not been able to carry out his plan of undertaking this investigation, he suggested that I should begin it and this paper may in a sense be regarded as a continuation of his spectral-photometric determination extended to the rays of greater wave-length.

Owing to the very numerous and unavoidable sources of error in such observations, due chiefly to our atmosphere, it seemed to be decidedly advantageous not to employ too delicate apparatus, and therefore, instead of using Langley's bolometer or Boy's radiomicrometer,—instruments whose extraordinary delicacy made it probable that these extraneous sources of error might largely mask the sought-for effect—I resolved also to employ the thermopile.

* From *Astronomische Nachrichten*, 3105-3106, with corrections and additions by the author.

† Spectralphotometrische Untersuchungen (Monatsberichte der Kgl. Acad. der Wissenschaften zu Berlin, März 1877). A valuable re-reduction and discussion of these observations is given by Professor Seeliger in his paper "Ueber die Extinction des Lichtes in der Atmosphäre" (Sitzungsberichte der math.-phys. Classe der Kgl. bayer. Acad. der Wiss. 1891 Bd. XXI, Heft III).

As, however, most of the thermopiles to be had of manufacturers are extremely short and do not allow of certainty that the back junctions do not receive heat by conduction from the exposed surface and do not permit that the back junctions may be kept at a constant temperature, I decided to construct the apparatus myself, a task which the admirable resources of this Observatory greatly lightened.

The original plan of observation was quite analogous to that with the spectral-photometer.

Twin thermopiles of considerable length with their back junctions at the same temperature were to be joined in circuit against each other and to be simultaneously exposed, the one directly to the Sun's rays, while the other, placed in the optical axis of the telescope, received the radiation from any given portion of the real projected solar image. Although this plan of observation had to be subsequently modified yet the construction of the apparatus was not altered.

A long time was spent in the attempt to make thermopiles of antimony and bismuth which should have a length of not less than 20 cm. and a sufficiently small diameter. The brittleness of these metals made it, however, practically impossible to fulfil these requirements and I was finally obliged to adopt iron and German silver for the purpose. These metals were procured in the form of wires of 0.3 mm. diameter with silk insulation; these were cut off in lengths of 25 cm., and two piles of 6 pairs each, as exactly similar as possible, were constructed and the exposed surfaces were brought into a plane occupying a space of about 4 sq. mm. A brass tube of 9 mm. diameter and 49 cm. length was then passed lengthwise through a tin cylinder of 9 cm. diameter and 16 cm. length, and the two ends were bent up at right angles so that the tube had a U shape. The back junctions of the two thermopiles, after having been carefully insulated and imbedded in sealing wax, were inserted into the two ends of the brass tube, the proportions being such that the front faces of the piles projected just out of the tube while the back junctions were in contact at the middle of the tube. The cylinder being now filled with water, (one liter), one could be assured that the two back junctions were at the same temperature, that no appreciable amount of heat could be transferred by conduction from the exposed faces, and that accidental thermo-effects in the metals of the thermopiles were practically impossible.

The upper ends of the brass tube pass into small wooden caps which are so hollowed out that the exposed faces lie at their

center, and over these were slipped pieces of polished brass tube 2 cm. long and 1 cm. diameter, blackened on the inside, and carrying on their upper ends cardboard diaphragms with an aperture of 5 mm. Other diaphragms of smaller aperture were inserted midway between these and the thermopile surfaces during some of the observations.

The whole of the apparatus up to the last mentioned brass caps was now protected against external radiation by being enclosed in a sort of cardboard box covered with tinfoil and then attached to a metal frame, constructed for a similar purpose previously, which was firmly clamped to the Grubb refractor of 20cm. aperture. This frame carried a pasteboard screen of 30cm. diameter, through the center of which the inner thermopile projected, and upon which were ruled off rectangular co-ordinates so that the distance of the point of the Sun's surface under examination from the center of the disk could be at a glance read off on the four radii. These were oriented to the apparent parallel to the daily motion except where it is noted that they were set for the Sun's true poles and equator.

The first method of procedure,—that of throwing different portions of the projected image upon the inner thermopile, while the outer (lying in the same plane but at a distance of 25 cm.) was simultaneously exposed to the direct rays,—had to be given up after a series of experiments because of the disproportionately greater intensity of the latter. By placing a thin silk gauze in the path of the direct rays and at a distance of several feet from the thermopile it was possible to secure the desired equality of intensity, as well as by inserting a shunt in the circuit against the outer pile, but both of the methods involved new and uncertain sources of error, and accordingly the plan of observation was modified, the inner pile being alone exposed. The outer pile was hereafter kept uniformly shaded from direct solar radiation, but served the important purpose of balancing all extraneous disturbing effects, such as air currents, reflected radiations, change of temperature of the water, etc.

It may be here remarked that the apparatus was as a rule brought to the dome a considerable time before the commencement of the observations in order that it might obtain the temperature of the air by the telescope. The exposure was made by rotating a shutter which was placed so far away from the thermopiles as to have no radiating effect itself; the objective was moreover kept closed as much as possible, being regulated by a convenient arrangement at the eye end. After some experi-

ments it was found that 30^s was the most advantageous time of exposure and it was uniformly so given, a clock in the dome with a loud tick furnishing the time. All the connecting wires at the telescope were well insulated and further enclosed in rubber tubing, which in turn was shielded from direct radiation by being covered with tinfoil. Exposed connections were protected by cotton batten.

The galvanometer, Siemens & Halske No. 1256, was of the astatic, dead beat type, and the deflection was observed by telescope and scale at a distance of generally 3 meters. The four coils were coupled in series. During the first of the observations, until Oct. 2, the galvanometer was set up on a bracket in the wall of the passage to the west dome, at a distance of about 5 meters from the telescope. It was soon found that the large amount of iron in the neighborhood exerted a very disturbing effect on the magnets, the zero point being constantly changed when the dome was moved, and it therefore became necessary to transfer the galvanometer to the physical laboratory, where a solid pillar gave a more steady support and where the neighboring iron masses were constant. This made a very long circuit unavoidable, but it was fortunately so situated as to be little exposed to changes of temperature.

The procedure of observation was this: First the assistant at the galvanometer gave the signal that the needle had come to rest, then a signal for attention would be returned, and 3^s before the exposure a second signal to make the reading was given, and then the shutter was opened with the clock beat. The galvanometer gave a steady throw during the 30^s and could be very exactly read off; it would have been possible to estimate the tenths of a mm. but it seemed best to only read the whole mm.

With a dead beat galvanometer of this sort the "first throw" cannot be accurately observed and so that method was not used.

It was always an important point that the needle should come to rest before making another exposure; very naturally it would not as a rule come quite back to the original zero-point and there was therefore a tendency for the zero-point to rise; in the small limits within which this occurred it could not be considered as introducing an error. With the distance of scale from mirror which was employed the heat was of course to be considered as directly proportional to the deflection.

The correction for torsion of the suspension thread was determined by turning the needle once around by means of a magnet and then observing the deflection from its normal position. The

torsion's factor varied according to circumstances from $\frac{1}{40}$ to $\frac{1}{100}$; the instrument being altered during the winter by increasing the length of the suspension. The data of the electrical circuit are: Resistance of the thermopiles each 8, of the galvanometer 10 ohms; of the rest of the circuit at first one ohm, and subsequently 7 ohms, making the totals in the two cases 27 and 33 ohms, certainly not the most advantageous combination, but under the circumstances necessary. The sensitiveness of the galvanometer was such that a deflection of one division of the scale was produced by a current of approximately 0.00000001 ampere (one hundredth of one millionth).

The detailed observations follow. They were made between 11 and 1 o'clock and only under the best atmospheric conditions, though they were occasionally interrupted by haziness or clouds. The low altitude of the Sun made observations impossible during the winter months.

The interval between successive exposures, determined by the time required for the needle to come fully to rest after a deflection, was usually about five minutes. The galvanometer readings were very carefully made by the clerk of the Observatory, Herrn Kettler. The quality of the exposure was always recorded at the telescope, and no observations have been here included by which inaccuracy as to the duration, position on the surface or atmospheric conditions were noted.

The first column contains the distance ρ, and direction (*N, E, S, W*, oriented to the parallel of declination), of the point observed from the center of the disk, the radius being taken as 100, and *M* denoting the center. The second and third columns give the corrected deflection in units of the scale and the ratio of this deflection (and consequently of the intensity) to that adopted for the center and designated by M_0 in the column of remarks; M_0 is the mean of the successive values for *M*, unless the notes ascribed less weight to one of the values. The difference in the absolute values on different days is due to the different diameters of the image employed and to the varying ratio of the temperature of the air (and water) to that of the Sun.

ρ	Defl.	$I:I_0$	Remarks.	ρ	Defl.	$I:I_0$	Remarks.
			1891, Sept. 12				Sept. 14 (I)
M	168		$M_0 = 168$	M	166		$M_0 = 165$
97 W	80	47.6		M	167		
50 W	149	88.7		74 W	140	84.8	
60 W	148	88.1		97 W	85	51.5	
74 W	142	84.5		87 W	117	70.9	
71 N	151	89.9		94 W	106	64.2	
87 N	128	76.2		50 W	160	97.0	
M	168			M	163		

ρ	Defl.	$I:I_0$	Remarks.
			Sept. 14 (II)
M	173		$M_0 = 173$
94 W	110	63.6	
97 W	87	50.3	
87 W	128	74.0	
M	172		
25 W	171	97.7	$M_0 = 175$
50 W	164	93.7	
74 W	139	79.4	
87 W	132	75.4	
94 W	112	64.0	
97 W	98	56.0	
M	178		
			Sept. 23
M	149		
94 W	98	66.2	$M_0 = 148$
94 S	92	62.6	
94 N	100	67.6	
M	147		
			Sept. 24
92 N	139	65.3	$M_0 = 213$
M	205		
92 E	149	70.0	
92 W	142	66.7	
92 S	135	63.4	
M	220		
			Sept. 25.
66 W	127	57.2	$M_0 = 222$
69 E	126	56.8	
96 S	126	56.8	
96 N	130	58.6	
M	224		
96 S	155	55.4	$M_0 = 280$ (Occasional Clouds)
96 N	168	60.0	
M	290		
96 W	176	62.9	
96 E	162	57.9	
M	270		
			Sept. 30
87 W	111	79.3	$M_0 = 840$
87 W	106	75.7	
M	142		
94 S	87	62.1	
" N	88	62.9	
" E	88	62.9	
" W	87	62.1	
M	135		Slight haze
M	134		
			Oct. 1
M	110		
97 W	60	54.1	$M_0 = 111$
97 E	58	52.3	
96 S	63	56.8	
M	111		
97 N	62	55.9	
25 W	111	100.0	
25 E	107	96.4	
50 E	105	94.6	
87 E	81	73.0	
M	112		
87 W	78	70.3	

ρ	Defl.	$I:I_0$	Remarks.
			Oct. 2
94 W	50	67.6	$M_0 = 74$
71 W	66	89.2	
60 W	68	91.9	
40 W	80	100.4	
97 W	43	54.0	$M_0 = 79.7$
M	81		
40 W	79	99.1	
97 W	43	53.9	
" N	46	57.7	
" S	43	53.9	
" E	44	55.2	
M	80		
M	78		
			Oct. 5
M	154		
76 N	131	86.8	$M_0 = 151$
" S	130	86.1	
" W	127	84.1	To-day oriented to the Sun's Pole.
" E	128	84.8	
M	147		
M	164		
96 N	88	56.4	$M_0 = 156$
" E	85	54.5	
" S	85	54.5	
" W	86	55.1	
M	154		
M	150		
			1892. March 30
M	117		
40 W	112	96.6	$M_0 = 116$
40 W	107	92.3	
76 W	86	74.1	
76 W	90	77.6	
M	115		
50 W	99	91.7	$M_0 = 108$
50 W	101	93.5	
76 W	82	75.9	
M	102		
			March 31
M	125		
50 W	114	91.2	$M_0 = 125$
75 W	99	79.2	
75 E	98	78.4	
			Clouds
50 E	107	90.7	$M_0 = 118$
94 E	72	61.0	
95 E	95	80.5	
M	118		
			April 5
50 W	121	94.5	$M_0 = 128$
M	129		
50 E	121	94.5	
75 E	103	80.5	
97 E	71	55.5	
M	127		
97 W	63	51.6	$M_0 = 122$
M	122		
M	122		
97 W	63	51.6	

ρ	Defl.	$I:I_0$	Remarks.
			May 9
M	166		
50 E	148	89.2	$M_0 = 166$
87 W	109	65.7	
M	166		
87 E	118	71.1	
75 W	129	80.6	$M_0 = 160$
" E	136	85.0	
" N	136	85.0	
" S	134	83.8	
" S	128	80.0	
M	148		
M	139		
87 N	101	73.7	$M_0 = 137$ (Oriented to the Sun's Pole)
87 S	87	63.5	
97 S	69	50.4	
96 N	77	56.2	
M	132		

ρ	Defl.	$I:I_0$	Remarks.
			May 9
87 N	91	69.0	$M_0 = 132$
" S	92	69.7	
" S	93	70.5	
" N	95	72.0	
M	132		
			May 12
M	141		
50 E	137	97.2	$M_0 = 141$
25 E	136	96.5	
25 N	130	98.5	$M_0 = 137$
50 N	126	95.5	
M	132		
25 S	134	100.2	$M_0 = 132$
50 S	124	93.9	
M	130	69.7	
87 S	92	73.5	
N	97		
87 N	97	73.5	To Sun's Pole
S	94	71.2	
S	89	67.4	
M	133		

The results of these observations are combined as follows:

ρ	97	96	94	92	87	76	75	74	71	60	50	40	25
No. of obs.	16	14	12	4	21	7	9	3	2	2	14	4	6
Mean $I:I_0$	53.2	57.1	63.9	66.4	71.7	81.3	81.4	82.9	89.5	90.0	93.3	97.1	98.8
Probable Error	± 0.44	0.40	0.42	—	0.53	1.35	0.55	—	—	—	0.47	—	0.43
P. E. of single obs.	± 1.8	1.5	1.5	—	2.4	3.6	1.7	—	—	—	1.8	—	1.1
Curve — Obs.	+1.7	+0.5	—2.1	— 1.2	0	—0.4	+ 0.2	—0.6	—5.5	—0.2	+0.3	—0.8	+ 0.1

Through these values (given in the third line) a smooth curve has been drawn, abscissas representing the distance from the center of the disc (ρ) and ordinates expressing the amount of heat transmitted, that at the center being taken as 100. The last line, Curve minus Observation, shows how well the curve satisfies the observations.

From the curve I now take the following values, given in the column headed O.

ρ	θ	O	C	C—O
0	0°	100.0	100.0	0.0
10	5.7	99.9	99.8	— 0.1
20	11.5	99.4	99.3	— 0.1
30	17.5	98.4	98.4	0.0
40	23.6	96.3	97.1	+ 0.8
50	30.0	93.6	95.1	+ 1.5
60	36.9	89.8	92.2	+ 2.4
70	44.4	84.6	87.8	+ 3.2
80	53.1	77.9	80.6	+ 2.7
90	64.2	68.0	65.6	— 2.4
100	90.0	(39)	—	—

$$\nu = 0.1412 \qquad e^{-f} = I_0 = 0.72.$$

In comparing now the above results with the Theory of Absorption we proceed from the standpoint that the Sun, deprived of its atmosphere, would appear as a flat uniformly illuminated

disc, as has been experimentally shown to be the case for glowing balls of metal. The formula which La Place gave in Méc. Célest. Book K, corrected to accord with this more modern view, becomes $I = e^{-f \sec \theta}$ where I represents the amount of light transmitted through the Sun's atmosphere, e the base of natural logarithms, f the coefficient of absorption, and θ the angle at the Sun's center between the line to the observer and the radius to the point observed, ρ being the sine of θ. At the center of the disc where $\theta = 0$ the intensity is $I_0 = e^{-f}$. The above measurements are therefore a determination of the ratio $\frac{I}{I_0} = \frac{e^{-f \sec \theta}}{e^{-f}}$; this formula may be more conveniently expressed in the form

$$\log \frac{I}{I_0} = - \nu \frac{1 - \cos \theta}{\cos \theta}$$

where $\nu = f \times$ Mod.

Using this formula I have computed ν for each of the given values of ρ and have then determined the most probable value of ν by the method of least squares, and finally have substituted this value of ν in the formula, and thus calculated the values of $I : I_0$ given in the fourth column of the table under the heading C. The column C — O shows the comparison of the theory with the observations. The differences are similar to those in the spectral-photometric observations, especially for green rays, attaining a maximum for about $\rho = 70$; their amount is greater here, as was to be expected, owing to the numerous unavoidable sources of error in such heat measurements; yet the departure of the observed curve from that derived from the theory seems to be real, and would indicate the insufficiency of the formula to absolutely represent the observations.

This divergence could be doubtless much diminished by the introduction of another constant in the formula, as Professor Seeliger has done for the green, blue, dark blue and violet rays, but did not find necessary for the red and yellow rays. I have not been disposed to do so for the present measurements, since the amount of the differences C — O are really very small, and the coincidence thereby gained would be rather illusory. The physical interpretation of the additional constant is moreover somewhat uncertain.

I may say here that my attempts to find a certain amount of radiation from the absorbing layer itself have led to negative results.

A comparison of the above results with the spectral-photometric observations is now of interest:

$\lambda =$	662 $\mu\mu$	......	579	513	470	443	409	......
ρ	Red	Heat	Yellow	Green	Blue	Dark-blue	Violet	Photo-graphic
10	99.9	99.9	99.8	99.7	99.7	99.7	99.6	99.6
20	99.5	99.4	99.2	98.7	98.8	98.7	98.5	98.4
30	98.9	98.4	98.2	96.9	97.2	96.8	96.3	96.7
40	98.0	96.3	96.7	94.3	94.7	94.1	93.4	93.7
50	96.7	93.6	94.5	90.7	91.3	90.2	88.7	89.7
60	94.8	89.8	90.9	86.2	87.0	84.9	82.4	83.3
70	91.0	84.6	84.5	80.0	80.8	77.8	74.4	73.7
80	84.3	77.9	74.6	70.9	71.7	67.0	63.7	59.6
90	71.0	68.0	59.0	56.6	57.6	50.2	47.7	39.3
*(100	30.0	39	25.0	16.0	16.0	14.0	13.0	13.5)

The heat curve thus lies, as might have been expected from the known position in the spectrum of the maximum of intensity for thermal rays, between those for red and yellow, only falling under the latter for the three middle values of ρ.

As the original publication of this extremely interesting research of Vogel's is inaccessible to many readers, a short account of the methods employed may not be out of place.

The form of spectral-photometer, as devised by Dr. Glan and modified by Dr. Vogel, consists of a compound spectroscope of the Bunsen type, with its slit divided into two halves by a small strip crossing it at the middle. Between the collimator lens and the prism are inserted first a doubly-refracting Wollaston prism and then a Nicol's prism which may be rotated on a graduated circle.

The doubly-refracting prism furnishes four images of the slit, two of which are gotten rid of by the insertion of a movable diaphragm in the focus of the observing telescope. The two remaining images of the slit, *i. e.*, spectra lying in contact one above the other, are polarized at right angles to each other, and consequently by rotating the Nicol they may be quite accurately made of equal intensity, since the brightness of the one increases while that of the other decreases. The adjustable diaphragm enables narrow and equal portions of the spectra to be compared. If now the light of the object be thrown upon one half of the slit, while the other half is illuminated by a standard source of light, we shall be able to compare the intensities of the two sources in different portions of the spectrum. In this case Vogel used the direct light of the Sun, thrown on one-half of the slit by a mirror and reflecting prism, as a standard source.

The primary image of the Sun from the 9-in. Berlin refractor was now projected upon the half-slit, and different portions of the image were brought upon it by means of the slow motion in declination.

*The values for $\rho = 100$ are, of course, quite uncertain, having been obtained by extrapolation.

In this way a series of comparisons was made between the intensity for the different colors at the center of the disc and at different points along the radius, and finally the results were combined in a curve.

The absorption for the photographically active rays was studied by Dr. Vogel (1872) on negatives of the Sun, the "density" of which at different points was measured by comparison with certain photographically prepared scales.

A long and careful series of measurements with the thermopile was made by Professor Langley in 1873-4. The investigation does not appear unfortunately to have ever been published in full; it is referred to in his paper "The Solar Atmosphere, an introduction to an account of Researches made at the Allegheny Observatory" (Am. Jour. X 1875), and in Comptes rendus, T. 80 and 81 (1875); in the latter he gives the amounts of the transmitted heat for four points of the disk. They are as follows:

ρ	No. obs.	Intensity ($I : I_0$)	Langley—F.	P. E. of single obs. Langley.	Frost.
50	72	95.0 ± 0.35	+ 1.4	± 3.0	± 1.8
75	98	85.9 ± 0.17	+ 4.3	1.7	1.7
96	33	61.9 ± 0.39	+ 4.2	2.2	1.5
98	124	50.1 ± 0.23	− 1.6	2.6	—

The discrepancies here are so much larger than the errors of observation indicated by the probable errors that they can be explained only by systematic errors on the part of one or both observers, or by an actual change in the transmission curve for the Sun. Considering the character of such observations, the former supposition seems the more probable, and the true values perhaps lie between these, although I am unable to account for any systematic errors in my observations which should tend to give too small results. Professor Vogel has given (Spect. phot. Untersuch.) a series of values taken from a curve which he constructed from Secchi's and a few of his own observations with the thermopile; he calls attention to their uncertainty. Cruls and Lacaille published in Comptes rendus, T. 88, 1879, the results of their thermopile measurements between Jan. 9 and 24, 1878.

They found the heat radiated from the Southern hemisphere of the Sun to be only three-fourths of that from the Northern.

ρ	Vogel & Secchi	Cruls N	Cruls S	VS—F.
10	100	—	—	0
20	99	97.5	80.0	0
30	99	91.7	63 9	+ 1
40	98	88.8	60.7	+ 2
50	97	82.3	57.6	+ 3
60	94	77.4	55.1	+ 4
70	89	67.7	52.1	+ 4
80	82	64.2	47.6	+ 4
90	69	50.5	39.9	+ 1
100	(40)	—	—	

These observations at Rio Janeiro, of which the details are not published, are not reconcilable with those of the other observers.

It is an interesting point to see whether the observations indicate a difference in the thermal conditions for the poles and equator and for the Northern and Southern hemispheres. Secchi announced that the Northern radiated the more and that the regions above the 30th parallel on the Sun radiated 6 per cent less than at the equator; this amount however, lies inside the range of errors of his observations. Langley found that the absorption was precisely the same along the four radii N, S, E and W. Accordingly I oriented the co-ordinate axes on the projecting screen parallel to the daily motion, for the obvious convenience in using the slow motions of the instrument; on three days, in order to test this point, I adjusted to the Sun's pole and equator. The resulting average difference in the transmission for two corresponding positions $N - S$ was less than one and one-half per cent, that for the Northern hemisphere being the greater; if all the observations be used (whether oriented parallel to the Earth's or Sun's equator) the average difference becomes $+ 1.9$; the values for corresponding points on the E, S and W radii differ on the average by less than one-half of one per cent. We may therefore conclude that the heat transmitted from the neighborhood of the Sun's poles is at present practically the same as that from a point on the equator equally distant from the center of the disc, and that the difference between the Northern and Southern hemispheres, if real, is exceedingly small.

As above stated, the computations gave the most probable value of $\nu = 0.1412$, whence the coefficient of transmission of the solar atmosphere, which is the intensity for $\theta = 0$ expressed as a fraction of the intensity ($= 1$) if there were no absorption, becomes 0.72, or in other words only 28 per cent of the heat radiation emitted from the Sun and passing along its radius is absorbed in its atmosphere. The spectral-photometric observations gave for this coefficient for the red rays ($\lambda = 662\ \mu\mu$) 0.77 and for the yellow rays ($\lambda = 579$) 0.66.

We pass to the consideration of the question: how much more heat should we receive from the Sun if its atmosphere were removed? This might be determined by integrating the expression already given for the intensity at any point of the surface, $I = e^{-f \sec \theta}$, which as we have seen nearly represents the observations. It is, however, more convenient to divide the Sun's disc into concentric zones of 0.05 of the radius in width, and to multiply the area of each zone by the intensity at its middle point

taken directly from the curve of observations, and then sum up these products. According to our original assumption that the Sun, deprived of its atmosphere, would send out its radiations to us equally from all portions of its disc, we should have (the radiation at the center, and the radius being each called unity) for the expression for the total heat radiation $I \pi R^2 = \pi = 3.14$.

The above process of summation, however, gives 2.56; moreover we have found that but 0.72 of the emitted heat is transmitted at the center; accordingly, were its atmosphere removed, the amount of heat received by us from the Sun would become $\frac{3.14}{2.56} \times \frac{1}{0.72} = 1.70$ times greater.

Professor Vogel found by this method for the red rays 1.54 and for the violet rays 2.67, as the factors by which these luminous radiations would be increased in the absence of a solar atmosphere. Secchi's results (computed after the uncorrected formula of Laplace): 8 for this last mentioned factor and 0.32 for the coefficient of transmission, may be mentioned for their historical interest. Langley has given no precise statement of his results, remarking that "not greatly less or more than one-half of the whole luminous heat rays" are transmitted through the solar atmosphere, so that this factor would be about 2 according to his observations.

II. Observations on Sun-Spots.

The apparatus remained unchanged for measurements of the thermal condition of Sun-spots. Had this been the chief object of this investigation certain alterations would have been made in the arrangement of apparatus. The much greater magnification which had to be employed for spots necessarily reduced the deflections of the galvanometer and this, as well as other obvious reasons, made the measurements much less accurate than those above given. As, however, both kinds of observations were frequently made on the same day, practical considerations decided me to leave the thermopiles and galvanometer unchanged.

In order to give a clear idea of the character of the observations I add the full details.

The first column explains itself. Professor Spoerer has kindly furnished me the heliographic latitudes and longitudes of the spots, as well as the values of ρ found in the second column. *n* and *p* refer to nucleus and photosphere, the point of the latter which was observed being always taken as near the spot as possible and so chosen as to be at the same distance from the center

of the disc. The deflection, corrected for torsion, is given in the third column, and the fourth contains the percentage of the thermal radiation from the nucleus of the spot to that from the neighboring photosphere, the mean value of which, p_0, is given in the first column.

In cases where a pause occurred during the observations this percentage is taken as the ratio of two successive measurements of n and p, and is indicated by brackets. Finally the mean value of the percentage or relative intensity i, is given in the first column followed by its value, i', when reduced to the center of the disc by means of the curve of absorption.

Detailed Observations.

	ρ		Defl.	i
1891 Sept. 12	80	p	140	
Spot *a*. $\beta = +14^\circ$ $\lambda = 130^\circ$		n	123	88
$i = 88$ $i' = 69$				
	48	n	115	72
Sept. 14 (I)		p	167	
Spot *a*. $p_0 = 160$		p	157	
		n	118	74
$i = 73$ $i' = 69$		p	157	
		n	119	74
Sept. 14 (II)	48	n	29	66
Spot *a*. $p_0 = 44$		p	46	
		p	41	
$i = 70$ $i' = 66$		n	32	73
		p	44	
Sept. 24	97	n^*	41 }	111
Spot *b*. $\beta = +23^\circ$ $\lambda = 305^\circ$		p	37 }	
		p	30 }	103
$i^* = 107$ $i' = 59$		n	31 }	
Sept. 24	92	n	40	114
Spot *c*. $\beta = -23^\circ$ $\lambda = 86^\circ$		p	34	
$p_0 = 35$		p	36	
$i = 116$ $i' = 75$		n	41	117
Sept. 25	97	p	27	
Spot *c*. $p_0 = 26$		n^*	25	96
		n	27	104
		p	25	
$i^* = 96$ $i' = 53$		p	26	
		n	23	88
The same.		p	9	
Highest power eye-piece.		n	7	117
$p_0 = 6$		p	5	
		n	7	117
$i = 117$ $i' = 64$		p	5	
Mean of both $i = 106$ $i' = 58$				
Sept. 25				
Spot *b*. Western Nucleus.	91	p	39	
$p_0 = 37$		n	26	70
		n	27	73
		p	36	
$i = 75$ $i' =$		p	36	
		n	30	81

	ρ	Defl.	i
Oct. 2			
Spot *d*. $\beta = +27°$ $\lambda = 249°$	58 *p*	24	
$p_0 = 24$	*n*	21	89
	n	18	76
	p	24	
$i = 82$ $i' = 74$	*p*	23	
	n	19	80
Oct. 5			
Spot *e*. $\beta = -16°$ $\lambda = 198°$	70 *p*	26	
Western Nucleus. $p_0 = 26$	*n*	21	81
	n	19	73
$i = 77$ $i' = 65$	*p*	26	
Oct. 5			
Spot *d*. $p_0 = 27$	37 *n*	23	85
	p	27	
	p	27	
	n	22	81
$i = 81$ $i' = 79$	*n*	21	78
	p	27	
Oct. 7			
Spot *d*.	60 *p*	26	88
	n	23	
	p	24	79
	n	19	
$i = 85$ $i' = 77$	*p*	54	87
	n	47	
Oct. 9			
Spot *e*. $p_0 = 49$	45 *n*	42	86
	p	50	
	p	48	
	n	36	73
$p_0 = 42$	*p*	43	
	n	33	79
$i = 80$ $i' = 76$	*n*	35	83
	p	41	
Oct. 10			
Spot *f*. $\beta = +13°$ $\lambda = 130°$	68 *p*	39	
contains a "Bridge"	*n**	33	85
$p_0 = 39$	*n*	33	85
	p	38	
$i^* = 85$ $i' = 73$	*p*	39	
	n	33	85
Oct. 10			
Spot *e*.	57 *n*	32	85
contains a "Bridge"	*p*	38	
$p_0 = 38$	*p*	37	
$i = 81$ $i' = 74$	*n*	29	77
Oct. 10			
Spot *g*. $\beta = -15°$ $\lambda = 209°$	67 *n*	27	84
Western Nucleus.	*n*	24	76
	p	32	
$p_0 = 31.7$	*p*	31	
	n	26	82
$i = 83$ $i' = 72$	*n*	28	88
	p	32	

	ρ	Defl.	i
Oct. 16			
Spot *f.* (bridged)	60 *p*	43	
$p_0 = 44$	*n*	37	85
	n	37	85
$i = 85 \quad i' = 77$	*p*	44	
1892 March 21			
Spot *h.* $\beta = +10^\circ \quad \lambda = 169^\circ$	29 *p*	58	
	n	48	73
$p_0 = 66$	*p*	70	
	n	49	74
$i = 74 \quad i' = 73$	*p*	69	
	n	49	74
March 22			
Spot *h.* N. W. Nucleus.	34 *p*	62	
	n	35	60
	n	42	72
$p_0 = 59$	*p*	56	
	p	57	
	n	49	83
	n	47	80
$i = 74 \quad i' = 73$	*p*	62	
	n	43	73
	p	57	
March 22			
Penumbra of Spot *h.*	34 *pen*	46	77
$p_0 = 60$	*pen*	47	78
	p	61	
$i = 78 \quad i' = 76$	*p*	59	

* The star indicates that a portion of the penumbra was probably included with the nucleus.

A rather surprising result of these observations was that spots are occasionally relatively warmer than the surrounding photosphere.

Unless the air was very steady, it was difficult to be absolutely sure that no portion of the penumbra was included with the nucleus; were it the case, however, it would scarcely account for a radiation exceeding that of the neighboring photosphere unless it be a real condition. I am therefore forced to conclude that the observations represent the true state of affairs. The question at once arises whether these differences of temperature depend upon the distance of the spot from the center of the disc, and a reference to the observations shows that the two spots with the highest relative temperature were very near the Sun's edge. The weather has unfortunately not permitted following a spot from day to day as it approached the edge, and days clear enough for observations have been scarce. While it would be absurd to attempt to draw too general conclusions from the few measurements made here, yet it is of interest to see if the spots are subject to the same law of absorption as the photosphere, since we may perhaps hereby gain some idea of their position (depth) in reference to the photosphere.

Accordingly I give the following comparison of the results for different days, the radiation of the nucleus being respectively referred to that of the surrounding photosphere (i), and to that of the center of the disc (i').

	a			*b*			*c*			*d*	
ρ	i	i'	ρ	i	i'	ρ	i	i'	ρ	i	i'
80	88	69	97	107	59	92	116	75	58	82	74
48	72	68	91	75	50	97	106	58	37	81	79
									60	85	77
	e			*f*			*g*			*h*	
70	77	65	68	85	73	67	83	72	29	74	73
45	80	76	60	85	77				34	74	73
57	81	74									

The values of i represent the temperature ratio of spots to neighboring photosphere if the absorption is the same for both, those of i', on the contrary, if the spots suffer no absorption (more than at the center of the disc, *i. e.*, no absorption which is a function of ρ). While now the observations are unfortunately not sufficient in number to allow of more than stating what they suggest, I am inclined to think that the true condition lies between the values of i and i', but more nearly the latter, since the values of i' coincide somewhat better among themselves and for different spots than those of i; since the values of i' do not show the falling off in intensity as the spot nears the limb (except for spot *c*, which was poorly determined on the second day), which would be expected if the same amount of absorption took place for spots and photosphere, and finally since otherwise we should be obliged to believe that spots near the limb not infrequently reach a higher temperature than the surrounding photosphere. If these data were sufficient to absolutely establish that the spots are subject to a considerably less absorption than the neighboring photosphere, then it would seem most readily accounted for by considering them to lie in a higher stratum than the photosphere. In C. R. Tome 80, p. 848 (1875) Langley remarks as follows: "· · · Avec de plus grandes images et un appareil perfectionné, je trouvai que, dans un anneau complet de la surface solaire, la photosphère encore brilliante donnait près du bord absolutement moins de chaleur que le noyau des taches. Il me fallut beaucoup de temps pour établir ce fait d'une manière incontestable, car cet intéressant phéonmène ne peut être bien observé qu'a moins de ½ minute d'arc du limbe, et des précautions particulières devaient être prises pour empêcher qu'aucune vacillation de l'image n'affectât les mesures."

According to my observations the thermal radiation from the nucleus of an average spot is about the same as that from an

equal area on the Sun's surface at a distance of nine tenths of the radius ($\rho = 88$) from the center of the disk.

The only exact measurements of the thermal conditions of Sunspots known to me and available for comparison were made by Langley in 1874-5 (*Monthly Notices* XXXVII p. 5). They are unfortunately not published in full. The method was similar to that employed here except that but one thermopile was used.

He states " . . . The quotient expresses the value of the umbral radiations, in parts of those of the adjacent photosphere. The decrement of heat, as we approach the limb, is, though not exactly, yet so very nearly, in the same ratio for photosphere and spots, that no correction is needed on this account for the present observations. 36 measurements on umbræ, and 32 on penumbrae were obtained in the autumn of 1874 and the spring of 1875. . . . The result is, that taking the mean thermal photospheric radiation in the spot's vicinity as unity, the mean umbral radiation is 0.54 ± 0.005, the mean penumbral 0.85 ± 0.01. These probable errors include all the discrepancies due to the greater or less approach to the limb in the spots measured, or due to absolute differences in their radiation, as well as the errors of observation."

The greatest and least values, among the 36 which are there given, are 0.63 and 0.43. Whether the separate measurements are of different spots on different days or include the single determinations for the same spot on one day or more is not stated. The observations must have been therefore made on spots which were not very far from the central portions of the disc, for otherwise they would be rather contradictory to those above quoted from Comptes rendus.

Although it seems to me that the individual character of Sunspots is often so different as to make it scarcely allowable to combine them together in taking a mean value, yet for the sake of comparison I give the mean value for the above 17 day-values, reduced to the center of the disc, viz. 0.70 ± 0.01; referred to the surrounding photosphere this becomes 0.85 ± 0.02; 52 single measurements on the above given eight spots are included in these values.

Professor Langley's instrumental equipment was considerably better than that used here, and I have no doubt that his measurements on spots are superior to mine.

Furthermore, as already stated in the notes, there was sometimes danger that a small portion of the penumbra was included with the nucleus, so that there would be a tendency for my meas-

urements to be too large. I cannot believe, however, that the large difference 0.16, or 25 per cent of the whole quantity measured, can be thus accounted for.

It may be remarked that Prof. Langley's observations were made about three years after a solar maximum while these of mine at about two years after a minimum.

In our present state of knowledge of this subject it is impossible to assert that the thermal conditions of spots (and perhaps of the photosphere and atmosphere) are invariable during the eleven-year period of solar activity, and it is to be hoped that the importance of systematic observations on this subject may be more fully recognized by observers having favorable climates and adequate instruments.

In conclusion I must express my deep indebtedness to Professor Vogel for the resources he has so freely placed at my disposal and for the interested encouragement with which he has followed my work.

Potsdam, 1892, June.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory, of the University of Chicago, U. S. A. Authors of papers are requested to refer to page 752 for information in regard to illustrations, reprint copies, etc.

A New Outburst of Nova Aurigæ.—The following circular from the Wolsingham Observatory was our first intimation of the Nova's reappearance: "Mr. H. Corder having informed me that the Nova Aurigæ has increased, it was examined here Aug. 21 and found to be 9.2, spectrum monochromatic. One intense line (500?). T. E. Espin." It is safe to say that this announcement caused considerable surprise wherever it was received. At the Lick Observatory in April, Mr. Burnham had watched the gradual decline in the star's brilliancy until it was too low in the west for further observation. At that time it was almost at the limit of vision with the great telescope, and it seemed probable that it would soon be lost to view. Of early morning observations since that time we have no information, and it seems probable that no one saw the star pass its minimum, and rise again into an easily visible object. In fact, the most generally accepted explanation of the Nova's first outburst is perhaps to some degree responsible for the subsequent lack of observations. For the two bodies were supposed to be moving in hyperbolic orbits after their close approach, and consequently no second meeting could ever occur. But not only does the new outburst of light make necessary a revision of our ideas as to the form of the orbits: still another difficulty arises from the discovery that we are no longer dealing with a star, but with a nebula. On another page will be found observations which show that the spectrum has undergone a complete change, and is now quite of the type charac-

teristic of nebulæ. Moreover, the nebulosity surrounding the stellar nucleus is plainly visible to Mr. Barnard with the 36-inch Lick telescope, and he has found its diameter to be 3″. Whether it can be seen with a smaller instrument we do not yet know; we observed the Nova on the morning of Sept. 6 with Mr. Burnham, using the 12-inch refractor of the Kenwood Observatory, and though the star was well seen (estimated by Mr. Burnham as 10.4 mag.) the bright moonlight would have completely hid even a bright nebulosity. From Science Observer *Special Circular, No. 97* we learn that Dr. A. Krueger of Kiel found the magnitude to be 9.2 on August 21, and 9.3 on August 31, so the star seems to have been decreasing in brilliancy. It is stated in the same circular that observations of the Nova at the Harvard College Observatory show a quite constant brightness at about 10.5 magnitude. It is greatly to be hoped that the brightness will not much decrease until a sufficient number of visual and photographic observations are obtained to fully acquaint us with the character of the spectrum. Professor Campbell's determination of a steady increase in velocity is of great interest and value, and perhaps our collected data may ere long be complete enough to allow a solution of this remarkable stellar problem.

Exceptional Solar Disturbance.—Owing to the frequent disturbances in the electrical and magnetic conditions on the earth, an account of a marked disturbance near the Sun's eastern limb, [position angle 130 degrees], observed by me on the morning of Thursday, August fourth, might be of interest to some of your readers. The instruments used were the six-inch equatorial of the Lick Observatory and a spectroscope belonging to the Chabot Observatory. This latter was without micrometer, so all displacements were estimated and not measured. My attention was first attracted by a marked distortion of the C line, at about $8^h\ 36^m$ Pacific Standard Time. Between $8^h\ 10^m$ and $8^h\ 20^m$, I had been watching the same part of the Sun, but had seen nothing but two small prominences about 10° to 20° from the point which afterwards became the center of disturbance. From the time that it was first noticed until the observations were discontinued, the violence of the disturbance steadily decreased. The following reversals were noted with a narrow tangential slit. A line was reversed at about wave-length 7065, [probably Young's first chromosphere line]. 6678.1 was brightly reversed. The C line was very much distorted on both sides and looked very much as in the sketch (which accompanied the letter.)

The displacements at *a, b, c* and *d* were estimated proportionally to the relative positions of the neighboring lines and from these displacements the velocities at those points were found to be as follows; at *a*, 83 miles per sec., at *c*, 73 miles per sec., both towards the earth; and at *b*, 73 miles per sec., at *d*, 146 miles per sec., both away from the earth. D_1 and D_2 each showed a bright spot or nodule on each side of the line, probably corresponding to *c* and *d* of the figure; both lines were very brilliantly reversed low down in the prominence. D_3 was violently disturbed, showing nearly but not quite so much motion as C and looking very much the same as the C line. 5316.8 [1474 *K*] was brilliant near the base of the prominence. b_1, b_2 brilliant; b_3 very bright in spots. b_4 less bright than the other two magnesium lines. b_1 reversed to a considerable height and comparatively quiescent, F very brilliant and very much like the C line. $H\gamma$ brilliant and showing considerable motion towards the violet. By $9^h\ 40^m$ nearly all disturbance had ceased. I examined the eastern limb of the Sun for spots but could see none in the neighborhood of this disturbance. H. C. LORD.

Professor Schuster's Address before the British Association.—At the very successful meeting of the British Association, which was held in Edinburgh early in August, we believe that there were no papers specially dealing with astro-physical subjects. Professor Schuster's address as President of the Section of Mathematics and Physics contained, however, some suggestions of such interest and value that we give them in full below:

"Some of the results recently brought to light by investigations on the discharge of electricity have interesting cosmical applications. Thus it is found that such a discharge through any part of a vessel containing a gas converts the whole gas into a conductor.* The dissociation which we imagine to take place in a liquid before electrolytic conduction takes place must be artificially produced in a gas by the discharge itself. We may imitate in gases which have thus been rendered conductive many of the phenomena hitherto restricted to liquids; thus I hope to bring to the notice of this meeting cases of primary and secondary cells in which the electrolyte is a gas. There are other ways in which a gas can be put into that sensitive state in which we may treat it as a conductor, and we have every reason to suppose that the upper regions of our atmosphere are in this state. The principal part of the daily variation of the magnetic needle is due to causes lying outside the surface of the earth, and is in all probability only an electro-magnetic effect due to that bodily motion in our atmosphere which shows itself in the diurnal changes of the barometer. A favorite idea of the late Professor Balfour Stewart will thus probably be confirmed. The difference in the diurnal range between times of maximum and times of minimum Sun-spots is accounted for by the fact that the atmosphere is a better conductor at times of maximum Sun-spots.

The mention of Sun-spots raises a point not altogether new to this section. Careful observation of celestial phenomena may suggest to us the solution of many mysteries which are now puzzling us. Consider, for instance, how long it would have taken to prove the universal property of gravitational attraction if the record of planetary motion had not come to the philosopher's help. And surely the most casual observation of cosmical effects teaches us how much we have yet to learn.

The statement of a problem occasionally helps to clear it up, and I may be allowed, therefore, to put before you some questions, the solution of which seems not beyond the reach of our powers.

1. Is every large rotating mass a magnet? If it is, the Sun must be a powerful magnet. The comets' tails, which eclipse observations show stretching out from our Sun in all directions, probably consist of electric discharges. The effect of a magnet on the discharge is known, and careful investigations of the streamers of the solar corona ought to give an answer to the question which I have put.†

2. Is there sufficient matter in interplanetary space to make it a conductor of electricity? I believe the evidence to be in favor of that view. But the conductivity can only be small, for otherwise the earth would gradually set itself to revolve about its magnetic pole. Suppose the electric resistance of interplanetary space to be so great that no appreciable change in the earth's axis of rotation could have taken place within historical times, is it not possible that the currents induced in planetary space by the earth's revolution may, by their electro-

* An experiment by Hittorf (*Wied. Ann.* VII, p. 614) suggested the probability of this fact, which was proved independently by Arrhenius and myself.

† The efforts of Mr. Bigelow have a bearing on this point, also some remarks which I have made in a lecture before the Royal Institution (*Proc. Roy. Inst.* 1891), but nothing decisive can be asserted at present.

magnetic action, cause the secular variation of terrestrial magnetism? There seems to me to be here a definite question capable of a definite answer, and as far as I can judge without a strict mathematical investigation the answer is in the affirmative.

3. What is a Sun-spot? It is, I believe, generally assumed that it is analogous to one of our cyclones. The general appearance of a Sun-spot does not show any marked cyclonic motion, though what we see is really determined by the distribution of temperature and not by the lines of flow. But a number of cyclones clustering together like the Sun-spots in a group should move round each other in a definite way, and it seems to me that the close study of the relative positions of a group of spots should give decisive evidence for or against the cyclone theory.

4. If the spot is not due to cyclonic motion, is it not possible that electric discharges setting out from the Sun, and accelerating artificially evaporation at the Sun's surface, might cool those parts from which the discharge starts, and thus produce a Sun-spot? The effects of electric discharges on matters of solar physics have already been discussed by Dr. Huggins.

5. May not the periodicity of Sun-spots, and the connection between two such dissimilar phenomena as spots on the Sun and magnetic disturbances on the earth, be due to a periodically recurring increase in the electric conductivity of the parts of space surrounding the Sun? Such an increase of conductivity might be produced by meteoric matter circulating round the Sun.

6. What causes the anomalous law of rotation of the solar photosphere? It has long been known that groups of spots at the solar equator perform their revolution in a shorter time than those in a higher latitude; but spots are disturbances which may have their own proper motions. Duner* has shown, however, from the displacement of the Fraunhofer lines, that the whole of the layer which produces these lines follows the same anomalous law, the angular velocity at a latitude of 75° being 30 per cent less than near the equator.† As all causes acting within the Sun might cause the angular velocity of the Sun to be smaller at the equator than at other latitudes, but could not make it greater, the only explanation open to us is an outside effect either by an influx of meteoric matter, as suggested by Lord Kelvin, or in some other way. If we are to trust Dr. Wilsing's result that faculæ which have their seat below the photosphere revolve in all latitudes with the same velocity, which is that of the spot velocity in the equatorial region, we should have to find a cause for a retardation in higher latitudes rather than for an acceleration at the equator. The exceptional behavior of the solar surface seems to me to deserve very careful attention from solar physicists. Its explanation will probably carry with it that of many other phenomena.

A National Physical Laboratory.—In an able paper on this subject (see the *Pedagogical Seminary,* Vol. II, No. 1,) Dr. Arthur Webster of Clark University urges that the United States Government should follow the example set by Germany, and establish a National Physical Laboratory on a scale commensurate with the importance of such an institution. The subject is one which has already received considerable attention in England, where the situation is very similar to that existing in this country. Professor Oliver Lodge was one of the first to call attention to it there, and his suggestions at the Cardiff meeting of the British Association a year ago were followed up this year at Edinburgh by a general discus-

* Oefvers. af Kongl. Veterrk. Ak. Forhandl., 47, 1890.

† Although the importance of M. Duner's results would make an independent investigation desirable, the measurements of Mr. Crew, who by a much inferior method arrived at other results, cannot have much weight as compared with those of Duner.

sion, in which Professor von Helmholtz, the distinguished Director of the National Physical Laboratory of Germany, was a prominent participant. The speakers found no difficulty in adducing weighty reasons in favor of the proposed measure, though Professor Fitzgerald's expression of doubt as to whether the House of Commons is sufficiently educated to understand that the advance of scientific work is of national value made plain the serious difficulty of accomplishing the desired result. But while it might be impossible to convince the House of Commons or our own House of Representatives that a large appropriation would be many times returned in the nation's advance in pure science, it would seem that the technical side of the subject might be urged with a better hope of success. The need of Government standardization of electrical instruments becomes daily more apparent, and Professor Webster has forcibly presented this aspect of the question in the paper to which we have alluded. We heartily second the views he has expressed, and hope that American physicists may unite to present this important subject to Congress.

Progress in Solar Photography at the Kenwood Observatory.—The sudden outburst on the Sun photographed at the Kenwood Observatory of the University of Chicago on July 15 having emphasized the importance of securing a practically continuous record of the condition of the solar surface, Professor Hale has devised a new form of spectroheliograph for this purpose. The instrument is radically different in design from the one now in use, and the form of construction is greatly simplified. An automatic arrangement has been added, which will allow photographs of the Sun, showing spots, faculæ and prominences, to be taken at any desired interval throughout the day, the instrument requiring no attention after once being set in operation.

The Distribution of Sun-Spots in Solar Latitude.—In the July number of *Knowledge* Mr. E. W. Maunder has an interesting paper under the above title. By means of a series of novel diagrams he illustrates the gradual change in latitude from minimum to maximum spot-period, and concludes "that these various relations—the sudden appearance of the spots of a new cycle in high latitudes, the persistent decline in latitude of the general spotted area as the cycle progresses, and the drift in latitude of individual groups—seem to me absolutely fatal to the idea, once popular, that the secret of solar disturbances lies without the Sun; in the relative positions of the planets, for example, or in the fall of meteorites." This view receives a strong confirmation, as Mr. Maunder points out, in the fact that outbreaks often recur in the same regions after considerable intervals of time.

New Observatories.—From *l'Astronomie* for August we learn that M. Bischoffsheim, the founder of the Nice Observatory, is about to construct an Observatory on the summit of Mt. Monnier in the Maritime Alps. The elevation is 2800 meters, 9200 feet, considerably greater than that of Nice.

A new Observatory, d'Abbas-Touman, has been opened in Trans Caucasia, in latitude 41° 46′ north, longitude 40° 32′ east from Paris. It was founded by the Grand-Duke Georges Mikhaïlowitch and is at a considerable elevation. M. Glasenapp, Professor of Astronomy in the University of St. Petersburg, has been charged with the mounting of the instruments and has already installed a ten-inch refractor.

CURRENT CELESTIAL PHENOMENA.

PLANET NOTES FOR NOVEMBER.

H. C. WILSON.

Mercury will be at greatest elongation east from the Sun, 21° 52′, Nov. 23. It will then set about an hour later than the Sun. Its low altitude will be unfavorable to day observations of the planet.

Venus will be in good position for observation just before sunrise. She will be in conjuction with the Moon Nov. 15 at $4^h\ 07^m$ P. M. We had a splendid view of this planet on the morning of Sept. 5. The phase was slightly crescent and at the cusps there were two oval white patches which seemed to stand out like the polar caps of Mars. Near the center a large dusky shading had the appearance of a real marking on the planet.

Mars during November will be getting pretty well toward the west in the evening, but as his declination will increase rapidly his position will be better for observation than it is at present. The diameter of his disk will decrease during the month from 14″ to 10″. Mars will be in conjunction with the Moon Nov. 27 at 11 A. M.

The months of August and September of course have been the great months for observation of Mars, and elsewhere in this journal will be found much concerning it, but the position of the planet will be more favorable to northern observers a little later, so that we may expect still further results. Clouds prevented us from observing the occultation of Mars on the morning of Sept. 4.

Jupiter will be in his best position for observation during the month of November, crossing the meridian at a high altitude early in the night. There will be two occultations of Jupiter during this month, the conjunctions occurring Nov. 2 at 5 P. M. and Nov. 29 at midnight, central time. These will be visible as occultations only in equatorial and southern latitudes. As seen in our latitude the Moon will pass to the south of Jupiter in both instances.

The great red spot has about the same appearance as during last year and is equally conspicuous. It came to the central meridian of the planet at the predicted time Sept. 14, so that the ephemeris which we give of its meridian passages may be considered correct.

One of the most important astronomical discoveries of this century has just been made by Mr. E. E. Barnard with the 36-in. equatorial of Lick Observatory, the discovery of a fifth satellite of Jupiter. Probably no planet has been examined more frequently and more thoroughly with telescopes of all kinds and sizes than has Jupiter. Yet no satellites have been added to the four which were discovered by Galileo with the first telescope in January 1610. The new satellite is of the 13th magnitude so that it can be seen only with large telescopes and with great difficulty because of the glare of the planet. Its greatest distance from the limb of the planet is about equal to the planet's diameter.

Saturn is a morning planet visible for about three hours before sunrise.

Uranus will be at conjunction with the Sun Oct. 29, and will be hidden by the solar rays during November.

Neptune is in good position for observation after midnight. He comes to opposition on the first day of December.

MERCURY.

Date. 1892.	R. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Nov. 5	15 51.4	− 22 09	8 20 A. M.	12 49.6 P. M.	5 19 P. M.
15	16 50.9	− 25 02	8 55 "	1 09.6 "	5 25 "
25	17 40.5	− 25 39	9 08 "	1 19.8 "	5 32 "

VENUS.

Nov. 5	12 12.5	+ 0 21	3 06 A. M.	9 11.2 A. M.	3 16 P. M.
15	12 56.7	− 4 03	3 28 "	9 15.9 "	3 03 "
25	13 41.8	− 8 26	3 52 "	9 21.7 "	2 52 "

MARS.

Nov. 5	22 04.1	− 14 01	1 54 P. M.	7 01.2 P. M.	12 08 A. M.
15	22 25.2	− 11 36	1 26 "	6 42.9 "	12 00 mdn
25	22 47.1	− 9 02	12 58 "	6 25.4 "	11 53 P. M.

JUPITER.

Nov. 5	1 04.5	+ 5 11	3 31 P. M.	10 01.1 P. M.	4 25 A. M.
15	1 01.0	+ 4 52	2 56 "	9 18.2 "	3 41 "
25	0 58.6	+ 4 40	2 15 "	8 36.7 "	2 58 "

SATURN.

Nov. 5	12 32.9	− 1 10	3 34 A. M.	9 32.8 A. M.	3 32 P. M.
15	12 36.7	− 1 33	3 00 "	8 57.3 "	2 55 "
25	12 40.3	− 1 54	2 28 "	8 21.4 "	2 20 "

URANUS.

Nov. 5	14 18.5	− 13 23	6 08 A. M.	11 18.1 A. M.	4 28 P. M.
15	14 20.9	− 13 35	5 31 "	10 40.2 "	3 49 "
25	14 23.2	− 13 47	4 56 "	10 03.3 "	3 11 "

NEPTUNE.

Nov. 5	4 36.9	+ 20 28	6 03 P. M.	1 32.9 A. M.	9 03 A. M.
15	4 35.8	+ 20 25	5 23 "	12 52.5 "	8 22 "
25	4 34.6	+ 20 23	4 43 "	12 12.0 "	7 41 "

THE SUN.

Nov. 5	14 45.4	− 15 59	6 44 A. M.	11 43.7 A. M.	4 43 P. M.
15	15 26.0	− 18 45	6 58 "	11 44.9 "	4 32 "
25	16 08.0	− 20 57	7 09 "	11 47.4 "	4 24 "

Minima of Variable Stars of the Algol Type.

U CEPHEI.

R. A. $0^h\ 52^m\ 32^s$
Decl. + 81° 17′
Period *2d* $11^h\ 50^m$

Nov. 4	6 P. M.
9	5 "
14	5 "
17	4 A. M.
22	4 "
27	4 "

ALGOL.

R. A. $3^h\ 01^m\ 01^s$
Decl. + 40° 32′
Period *2d* $20^h\ 49^m$

Nov. 3	6 P. M.
15	5 A. M.
18	2 "

ALGOL CONT.

Nov. 20	11 P. M.
23	8 "
26	4 "

R. CANIS MAJORIS.

R. A. $7^h\ 14^m\ 30^s$
Decl. − 16° 11′
Period *1d* $03^h\ 16^m$

Nov. 1	9 P. M.
3	1 A. M.
4	4 "
9	8 P. M.
10	11 "
12	2 A. M.
13	5 "
18	10 P. M.

R. CANIS MAJ., CONT.

Nov. 20	1 A. M.
21	4 "
26	9 P. M.
27	12 midn.
29	3 A. M.
30	6 "

Y CYGNI.

R. A. $20^h\ 47^m\ 40^s$
Decl. + 34° 15′
Period *1d* $11^h\ 56^m$

Nov. 1	11 P. M.
7	11 "
13	11 "
19	10 "
25	10 "

Phases and Aspects of the Moon.

		d	h m
Full Moon	Nov.	4	9 49 A. M.
Perigee	"	4	9 54 "
Last Quarter	"	11	4 02 P. M.
Apogee	"	17	11 06 "
New Moon	"	19	7 19 A. M.
First Quarter	"	27	10 28 "

Jupiter's Satellites.

	h	m		Sat.	Phenomenon
Nov. 1	12	14	A. M.	II	Sh. In.
	12	47	"	I	Tr. Eg.
	1	18	"	I	Sh. Eg.
	1	44	"	II	Tr. Eg.
	2	45	"	II	Sh. Eg.
2	5	02	P. M.	I	Tr. In.
	5	34	"	I	Sh. In.
	5	42	"	II	Oc. Dis.
	7	15	"	I	Tr. Eg.
	7	39	"	III	Oc. Dis.
	7	48	"	I	Sh. Eg.
	9	14	"	II	Ec. Re.
	9	42	"	III	Oc. Re.
	9	54	"	III	Ec. Dis.
	11	55	"	III	Ec. Re
7	3	09	A. M.	I	Oc. Dis.
8	12	29	"	I	Tr. In.
	13	01	"	I	Sh. In.
	1	35	"	II	Tr. In.
	2	33	"	I	Tr. Eg.
	2	53	"	II	Sh. In.
	3	14	"	I	Sh. Eg.
	9	35	P. M.	I	Oc. Dis.
9	12	28	A. M.	I	Ec. Re.
	6	47	P. M.	I	Tr. In.
	7	29	"	I	Sh. In.
	7	58	"	II	Oc. Dis.
	9	00	"	I	Tr. Eg.
	9	42	"	I	Sh. Eg.
	10	59	"	III	Oc. Dis.
	11	49	"	II	Ec. Re.
10	1	08	"	III	Oc. Re.
	1	56	"	III	Ec. Dis.
	6	57	P. M.	I	Ec. Re.
11	4	11	"	I	Sh. Eg.
	4	13	"	II	Sh. In.
	5	14	"	II	Tr. Eg.
	6	43	"	II	Sh. Eg.
13	6	12	"	III	Sh. Eg.
15	2	58	A. M.	I	Sh. In.
	11	22	P. M.	I	Oc. Dis.
16	2	23	A. M.	I	Ec. Re.
	8	33	P. M.	I	Tr. In.
	9	24	"	I	Sh. In.
	10	16	"	II	Oc. Dis.
	10	46	"	I	Tr. Eg.
	11	37	"	I	Sh. Eg.
17	2	23	A. M.	III	Oc. Dis.
	2	25	"	II	Ec. Re.
	5	49	P. M.	I	Oc. Dis.
	8	52	"	I	Ec. Re.
18	5	05	"	II	Tr. In.
	5	12	"	I	Tr. Eg.
	6	06	"	I	Sh. Eg.
	6	52	"	II	Sh. In.
	7	35	"	II	Tr. Eg.
	9	22	"	II	Sh. Eg.
20	4	14	"	III	Tr. In.
	6	28	"	III	Tr. Eg.
	8	01	"	III	Sh. In.
	10	13	"	III	Sh. Eg.
23	1	10	A. M.	I	Oc. Dis.
	10	20	P. M.	I	Tr. In.
	11	19	"	I	Sh. In.
24	12	33	A. M.	I	Tr. Eg.
	12	36	"	II	Oc. Dis.
	1	32	"	I	Sh. Eg.
	7	35	P. M.	I	Oc. Dis.
	10	48	"	I	Ec. Re.
25	4	47	"	I	Tr. In.
	5	48	"	I	Sh. In.
	7	00	"	I	Tr. Eg.
	7	28	"	II	Tr. In.
	8	01	"	I	Sh. Eg.
	9	31	"	II	Sh. In.
	9	59	"	II	Tr. Eg.
26	12	01	A. M.	II	Sh. Eg.
	5	17	P M.	I	Ec. Re.
27	6	18	"	II	Ec. Re.
	7	45	"	III	Tr. In.
	10	02	"	III	Tr. Eg.
28	12	03	A. M.	III	Sh. In.

Approximate Central Times when the Great Red Spot will pass the Center of Jupiter's Disk.

	h	m			h	m			h	m	
Oct. 16	6	44	P. M.	Oct. 31	4	05	P. M.	Nov. 15	11	24	P. M.
18	12	30	A. M.	Nov. 1	2	01	A. M.	16	7	15	"
18	6	22	P. M.	1	9	52	P. M.	18	1	2	A. M.
19	4	13	"	2	5	44	"	18	8	54	P. M.
20	2	08	A. M.	3	11	30	"	19	6	50	A. M.
20	10	00	P. M.	4	7	22	"	19	4	45	P. M.
21	5	51	"	6	1	09	A. M.	20	10	32	"
22	3	46	A. M.	6	9	00	P. M.	21	6	24	"
22	11	37	P. M.	7	4	51	"	23	12	11	A. M.
23	7	29	"	8	2	47	A. M.	23	8	2	P. M.
24	5	24	A. M.	8	10	38	P. M.	25	1	49	A. M.
25	1	16	"	9	6	29	"	25	9	41	P. M.
25	9	07	P. M.	11	12	16	A. M.	26	7	36	A. M.
26	4	58	"	11	8	8	P. M.	26	5	32	P. M.
27	2	54	A. M.	13	1	55	A. M.	27	11	19	"
27	10	45	P. M.	13	9	46	P. M.	28	7	10	"
28	6	36	"	14	7	42	A. M.	30	12	58	A. M.
30	12	23	A. M.	14	5	37	P. M.	30	8	49	P. M.
30	8	13	P. M.								

Configuration of Jupiter's Satellites at 10:30 p. m. Central Time

Nov.		Nov.		Nov.	
1	4 3 2 ○ ●	11	1 2 ○ 4 3	21	3 ○ 1 2 4
2	4 1 ○ 2 ●	12	2 4 ○ 1 3	22	3 2 1 ○ 4
3	○ 4 1 2 3	13	4 1 3 ○ 2	23	3 2 ○ 1 4
4	1 2 ○ 3 4	14	4 3 ○ 1 2	24	○ 3 2 4 ●
5	2 ○ 1 3 4	15	4 3 2 1 ○	25	♃ 1 ○ 3 4
6	3 1 ○ 2 4	16	♃ 4 3 2 ○	26	2 ○ 1 3 4
7	3 ○ 1 2 4	17	4 ○ 1 3 2	27	♃ 1 ○ 2 4
8	3 2 1 ○ 4	18	4 1 2 ○ 3	28	3 4 ○ 1 2
9	1 3 ○ 4 ●	19	2 4 ○ 1 3	29	3 4 2 1 ○
10	○ 1 2 4 3	20	1 3 ○ 2 4	30	4 3 2 ○ 1

Occultations Visible at Washington.

Date 1892.	Star's Name.	Magnitude.	IMMERSION Washington M. T. h m	IMMERSION Angle f'm N pt. °	EMERSION Washington M. T. h m	EMERSION Angle f'm N pt °	Duration. h m
Nov. 2	96 Piscium........	7	14 47	121	15 18	183	0 31
11	42 Leonis..........	6	11 07	143	11 49	249	0 42
13	*b* Virginis..........	6	14 58	106	16 04	313	1 06
14	Saturn...............		15 19	160	16 08	260	0 48
21	Σ Sagittarii.......	5	5 24	32	6 06	320	0 42
25	35 Capricorni....	6	5 31	59	6 52	239	0 21
27	ψ^2 Capricorni....	4	11 24	32	12 17	264	0 53
30	*o* Piscium..........	4	6 35	45	7 45	239	1 10

Occultation of Mars, Sept. 3, 1892.—This was observed at the Chamberlain Observatory by Mr. H. L. Shattuck, Mr. O. F. Shattuck and myself. Mr. H. L. Shattuck used a 2-inch telescope with a magnifying power of 30 diameters; Mr. O. F. Shattuck, a 5-inch with a power of 80, and I, a 6-inch with a power of 170.

The latitude and longitude are 39° 40′ 36″.4 and $6^h\ 59^m\ 47^s.63$ (from Greenwich). The local mean times of the phases are given below. At first contact, the telescope of O. F. S. was shaking so that he could not get distinct vision and estimated his time as 5 seconds slow. The atmospheric conditions were good.

	1st Contact. h m s	2nd Contact. h m s	3rd Contact. h m s	4th Contact. h m s
O. F. S.	10 45 40.4	10 46 29.1	12 2 10.8	12 2 53.8
H. L. S.		27.1	11.8	
H. A. H.	35.7	28.4	10.6	53.4

University Park, Colo. HERBERT A. HOWE.

Partial Eclipse of the Sun, Oct. 20.—We call attention again to this eclipse which was mentioned in our last number. The eclipse will be visible throughout almost the whole of North America. At Northfield the eclipse will begin at $10^h\ 32^m\ 53^s$ A. M. and end at $1^h\ 25^m\ 18^s$ P. M. central time. First contact will occur at a point 28.8° to the west, and last contact 97.6° to the east, of the north point of the solar disk. At the middle of the eclipse at Northfield about half the Sun's diameter will be covered by the moon. At Albany the eclipse will begin at $12^h\ 02^m\ 41^s$ and end at $3^h\ 03^m\ 39^s$ P. M. eastern time, the contacts occurring at points 34° west and 106° east from the north point of the Sun's disk.

New Minor Planet 1892 A (Wolf).—A new minor planet of the twelfth magnitude was discovered photographically by Wolf at Heidelberg. It was observed by Palisa at Vienna, Aug. 26, $10^h\ 52^m$: R. A. $22^h\ 42^m\ 16^s$; Decl. $-10°\ 22'$. Daily motion -14^s and $-3'$.

COMET NOTES.

Comet *d* 1892 (Brooks).—A new comet was discovered by Mr. W. R. Brooks of Geneva, N. Y., on the night of Aug. 28 in R. A. 5^h 59^m; Decl. 31° 52′. The discovery was verified on the following night and announced by telegraph on the next day. The announcement was received at Northfield Aug. 31 and the position of the comet determined on that night. The following observations are now at hand:

No	Date	Gr. M. T. h m s	R. A. h m s	Decl. ° ′ ″	Observer	Place
1	Aug. 31	20 07 46	6 06 50.44	+31 40 38.9	Wilson	Northfield
2	Sept. 1	17 49 43	6 09 01.66	31 35 51.2	Wendell	Cambridge
3	1	19 47 26	6 09 15.83	31 35 19.0	Wilson	Northfield
4	2	19 42 44	6 11 42.48	31 29 39.6	Wendell	Cambridge
5	4	2 29 45	6 14 45.7	31 22 50	Barnard	Lick Obs.
6	4	21 37 43	6 16 56.70	31 17 08.8	Wilson	Northfield

Since the last date cloudy weather and moonlight have prevented observations so that no good elements of the orbit are yet possible. The following rough elements were computed by Mr. A. G. Sivaslian and myself, using for the first set my own observations alone. As these gave very unequal intervals of time the results were not satisfactory. When the *Science Observer Circular* No. 97 arrived containing Mr. Wendell's observations, we made a new computation using the observations numbered 1, 4 and 6 and obtained the second set of elements:

	I	II	
T	= 1892. Dec. 22.80	1892, Dec. 9.31 Gr. M. T.	
π	= 165° 28′	182° 11′	
ω	= 263 27	284 37	1892.0
☊	= 262 06	257 35	1892.0
i	= 27 00	31 57	1892.0
q	= 0.7962	0.4764	

These elements agree as well perhaps as could be expected, considering the shortness of the arc described by the comet, only a little over 1° of heliocentric longitude, with those found by Berberich from observations Sept. 1, 4 and 6.

T	= 1892 Dec. 19.69 Gr. M. T.	
ω	= 269° 24′	1892.0
☊	= 261 03	1892.0
i	= 27 57	1892.0
q	= 0.6991	

It will be seen that there is considerable uncertainty as to the comet's perihelion distance, and therefore as to its brightness and visibility during the coming months. From the following ephemeris computed by Mr. Sivaslian from elements II, it seems certain that the comet will be much brighter than it now is.

Ephemeris of Comet *d* 1892 (Brooks).

Gr. M. T.	R. A. h m	Decl. ° ′	log Δ	log r	Brightness
Oct. 2.5	7 40.8	+25 25	0.1303	0.1752	4.5
6.5	7 56.4	+23 46	0.0999	0.1556	5.7
10.5	8 13.2	+21 46	0.0688	0.1348	7.2
14.5	8 31 5	+19 24	0.0369	0.1126	9.2
18.5	8 51.3	+16 32	0.0055	0.0892	11.9
22.5	9 13.0	+13 08	9.9746	0.0640	15.4
26.5	9 36.7	+ 9 06	9.9460	0.0370	19.9
30.5	10 02.7	+ 4 33	9.9214	0.0082	25.4
Dec. 9.3	15 34.5	−31 25	0.0757	9.6780	57 2

Ephemeris of Comet 1892 (Winnecke).

(From *Astr. Nach.* No. 3112).

		App. R. A.			App. Decl.		log r	log Δ	Br.
	h	m	s	°	′	″			
1892 Oct. 1	1	30	09.0	− 30	30	54			
2		28	12.8		21	08			
3		26	19.2		10	57			
4		24	28.4	30	00	21	0.2068	9.8395	0.807
5		22	40.4	29	49	21			
6		20	55.3		37	59			
7		19	13.1		26	15			
8		17	33.8		14	10	0.2176	9.8658	0.681
9		15	57.5	29	01	46			
10		14	24.1	28	49	03			
11		12	53.7		36	03			
12		11	26.3		22	46	0.2280	9.8922	0.574
13		10	01.9	28	09	13			
14		8	40.6	27	55	25			
15		7	22.2		41	24			
16		6	06.8		27	09	0.2382	9.9185	0.485
17		4	54.4	27	12	42			
18		3	44.9	26	58	04			
19		2	38.4		43	15			
20		1	34.9		28	16	0.2480	9.9448	0.411
21	1	0	34.3	26	13	08			
22	0	59	36.6	25	57	52			
23		58	41.8		42	28			
24		57	49.8		26	58	0.2576	9.9709	0.349
25		57	00.6	25	11	20			
26		56	14.2	24	55	38			
27		55	30.6		39	50			
28		54	49.6		23	58	0.2670	9.9967	0.297
29		54	11.2	24	08	03			
30		53	35.5	23	52	05			
31		53	02.3		36	04			
Nov. 1		52	31.5		20	01	0.2760	0.0222	0.253
2		52	03.3	23	03	56			
3		51	37.4	22	47	50			
4		51	13.9		31	43			
5		50	52.7	22	15	36	0.2849	0.0472	0.216
6		50	33.7	21	59	29			
7		50	16.9		43	22			
8		50	02.2		27	16			
9		49	49.7	21	11	11	0.2935	0.0718	0.186
10		49	39.2	20	55	07			
11		49	30.8		39	04			
12		49	24.4		23	03			
13		49	19.9	20	07	06	0.3019	0.0960	0.160
14		49	17.3	19	51	06			
15	0	49	16.6	− 19	35	10			

Comet 1892 I.—Swift's comet is still an easy object in small telescopes. On the evening of Sept. 20 it was quite conspicuous in our 5-inch finder. It has a well defined nucleus and short broad tail. It is now in the southern part of the constellation Cassiopeia and moving slowly southwest.

In *Astr. Nach.* 3110 Mr. Berberich gives elliptic elements of this comet depending on observations of March 8, April 10, May 12 and July 12. The best parabola gave residuals of 28″ and 20″ in the latitudes of the middle places, while the ellipse gave very small residuals.

On the other hand, Miss F. Gertrude Wentworth (*Astr. Jour.* No. 273), using observations of several observers on the dates March 7, May 4 and June 29, finds parabolic elements completely representing the observations. The following ephemeris calculated by Miss Wentworth is taken from *Astr. Jour.*, No. 274.

Ephemeris of Comet 1892 I (Swift.)

Gr. M. T.	App. R. A. h m s	App. Decl. ° ′	log Δ	Brightness
Oct. 1.5	0 03 38.9	+47 21.8		
2.5	2 36.4	47 05.9		
3.5	1 35.5	46 49.8		
4.5	0 00 36.3	46 33.5	0.3070	0.050
5.5	23 59 38.9	46 17.1		
6.5	58 43.2	46 00.4		
7.5	57 49.3	45 43.6		
8.5	56 56.7	45 26.7	0.3146	0.046
9.5	56 05.2	45 09.6		
10.5	55 15.4	44 52.2		
11.5	54 27.0	44 34.7		
12.5	53 40.7	44 17.1	0.3270	0.043
13.5	52 56.3	43 59.5		
14.5	52 13.7	43 41.8		
15.5	51 32.8	43 24.0		
16.5	50 54.0	43 06.1	0.3320	0.040
17.5	50 17.0	42 48.2		
18.5	49 42.0	42 30.2		
19.5	49 09.0	42 12.2		
20.5	23 48 38.1	41 54.7	0.3415	0.037

NEWS AND NOTES.

Foreign subscribers are reminded that money orders may be drawn on Northfield as its post office has recently been made a foreign money order office.

The Sidereal Messenger forms one series of this publication consisting of ten volumns covering a period from 1882 to the end of 1891. Astronomy and Astro-Physics began with January 1892. The volume for this year, the first in the new series, will contain about 1,000 pages with nearly fifty full page plate engravings.

The Planet Mars. We have given large space in this number, to recent studies of the planet Mars by some of the best astronomers in this country. Our readers will find it all most valuable matter, but wholly devoid of all that highly sensational and imaginative display of knowledge about the people of Mars which has appeared so constantly in the daily papers during the last sixty days.

Is Mars Inhabited?—Whether the planet is inhabited or not, seems to have been the all-absorbing question, everywhere in popular thought and expression. With the astronomer this query is almost the last thing about the planet that he would think of when he has an opportunity to study its surface markings at such a favorable opposition as that which has recently past. No astronomer claims to know whether the planet is inhabited or not. The chief thing that he is after is to see all he possibly can of detail markings on the planet's surface, measure and map as much as he can be sure of by repeated observation. This is painstaking, and very laborious work, courageously pursued every favorable night as long as the planet is within reach of the telescope. Under such circumstances the astronomer would not have much to say about the people of Mars, however pointedly he might be questioned. This brings to mind the report that has been circulated far and near, that we have said " Mars is undoubtedly inhabited " *We have never made any such statement.*

An Account of the Discovery of a Fifth Satellite to Jupiter.—I am glad to write some account of the discovery of the fifth satellite of Jupiter for ASTRONOMY AND ASTRO-PHYSICS, as telegraphically requested by the editor.

I have already sent to Dr. Gould for the *Astronomical Journal* all my micrometrical observations so far obtained of the satellite and a short account (which will be a historical record) of the conditions of the discovery. It is unnecessary for me to go into those same details here, as they will be found on record in the pages of the *Astronomical Journal*.

Friday being my night with the 36-inch telescope, after observing Mars and measuring the positions of his satellites, I began an examination of the region immediately about the planet Jupiter. At 12 o'clock as near as may be, to within a few minutes, I detected a tiny point of light close following the planet and near the 3rd satellite which was approaching transit. I immediately suspected it was an unknown satellite and at once began measuring its position-angle and distance from the 3rd satellite. On the spur of the moment, this seemed to be the only method of securing a position of the new object, for upon bringing the slightest trace of the planet in the field the little point of light was instantly lost.

I got two sets of distances and one set of position-angles, and then attempted to refer it to Jupiter but found that one of the wires of the micrometer was broken out and the other loose. Before anything could be done the object rapidly disappeared in the glare of Jupiter. From the fact that it was not left behind by the planet in its motion, I was convinced that the object was a satellite. A careful watch was kept at the preceding limb of the planet for the reappearance of the satellite, but up to daylight it could not be seen.

Though positive that a new satellite had been found, extreme caution suggested that it would be better to wait for a careful verification before making any announcement.

The following night with the 36-inch belonging to Professor Schaeberle, he kindly gave it up to me, and shortly before midnight the satellite was again detected rapidly leaving the planet on the following side. That morning I had put new wires in the micrometer, and now began a series of careful measures for position. As I have said, the satellite was so small that no trace of Jupiter could be admitted into the field for reference in the measures. It was necessary, therefore, to bisect the satellite, with the planet out of the field, and then by sliding the eye-piece bring the limb of Jupiter into view and bisect it. This method did not permit any measures from the polar limbs of Jupiter. Following the satellite thus, it was seen to recede from the planet to a distance of some 36" from the limb when it gradually became stationary. Remaining so for a while it began once more to approach the planet and rapidly disappeared in the glow near the limb. The measures, repeated as rapidly as possible, thoroughly covered the elongation, and gave the means of approximating to its period.

The following morning a telegram was sent out announcing the discovery. Subsequent observations have thoroughly confirmed the discovery.

On account of its extreme closeness to the planet it is difficult to say just what its magnitude is. Taking everything into account, I have provisionally assigned it as thirteenth magnitude. I hope to be able to settle definitely this question by observing some little star near Jupiter and then afterwards determining its magnitude when the planet has left it. Until this is settled, any estimate of the actual size of the satellite must be the merest guess, but it will probably be found to not exceed 100 miles in diameter, and perhaps less than that.

After the first few observations I inserted a piece of smoked mica in the eye-

piece, and using this as an occulting bar, the measures were made with ease and accuracy. Careful measures thus made from the polar limbs for the Jovicentric latitude of the satellite, show that its orbit lies sensibly in the plane of Jupiter's equator and that consequently the satellite is not a new addition to the Jovan family, since it would doubtless require ages for the orbit to be so adjusted if the object were a capture.

A sufficiently long interval has not yet elapsed to permit an accurate determination of the periodic time of the new satellite, but using three of the measured elongations and the known mass of Jupiter I have deduced the following approximations to the period, by the formula:

$$P = p\sqrt{\frac{m}{M}\,\frac{R^3}{r^3}}$$

where m and M are the masses of the Earth and Jupiter, and p and r the period and distance of our Moon, R in the three cases being derived from the direct measures and having the following values:

112,250 miles	Periodic Time = 11^h 47.6^m
112,750 "	11 52.3
112,400 "	11 49.0

Hence the mean of these—11^h 49.6^m—will not be far from the truth, as it seems to satisfy the mass of Jupiter.

It will be thus seen that this new satellite makes two revolutions in one day, and that its periodic time about the planet is less than two hours longer than the axial rotation of Jupiter. Excepting the inner satellite of Mars it is the most rapidly revolving satellite known. When sufficient observations have been obtained it will afford a new and independent determination of the mass of Jupiter. Of course from what I have said in reference to the difficulty of the new satellite, it will be apparent that the most powerful telescopes in the world, only, will show it.

E. E. BARNARD.

Mt. Hamilton 1892, Sept. 21.

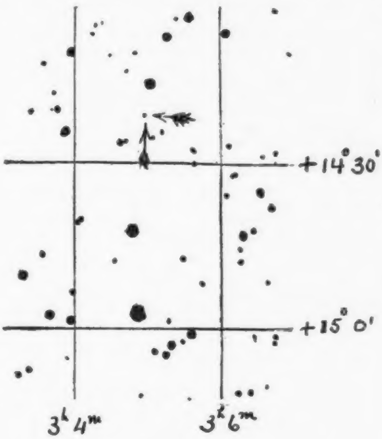
Nova Aurigæ.—Observations have been made of Nova Aurigæ at the Harvard College Observatory on every clear evening since its reappearance has been announced. In the spring it was last seen here on April 26, when it had the magnitude 14.5 Later observations were prevented by its low altitude in the evening twilight. Observations by Mr. O. C. Wendell with a photometer attached to the 15-inch equatorial on September 2, 6, 7, 8 and 9, gave the stellar magnitudes 10.62, 10.60, 10.57, 10.57 and 10.54. The change, if any, appears therefore to be very slight and to indicate an increase in light. These observations show that it is slightly brighter than the star following it at 3^s and $2'$ north, whose magnitude is found to be 10.94. A series of photographic charts, on the other hand, make it about half a magnitude fainter than this star, showing that its color is on the whole redder than that of the comparison star.

Its spectrum was last certainly photographed in the spring on March 24 when of the eleventh magnitude. On March 21, the hydrogen lines G, F, H, and h were shown in the order of brightness named. On September 2, two lines of nearly equal brightness were clearly shown in the photograph, one coinciding with the hydrogen line G, the other having a somewhat greater wave-length than F, and probably coinciding with the principal nebula line $\lambda = 500$. An interesting analogy is suggested in the observation of Lord Lindsay in 1877 that Nova Cygni then gave the spectrum of a planetary nebula (Observatory III, p. 185).

EDWARD C. PICKERING.

Harvard College Observatory, Cambridge, Mass., Sept. 12, 1892.

A New Variable Star in Aries.—With the aid of some photographic plates kindly lent to us by the Harvard College Observatory I have found a new variable star in the approximate position R. A. $3^h\ 49^m$; Decl. + 14° 22′ (1892).



NEW VARIABLE STAR IN ARIES.

On one of the two plates available the image corresponds to a star of about the 9.5 magnitude, while on the other plate the image if present, is fainter than the faintest images which can be recognized with certainty. Visual observations made with a six-inch telescope show a star of the eleventh magnitude in the proper position.

To make sure of the identity of the star I made an exposure of 60^m, with the Willard lens of the Crocker telescope, on Aug. 26. The photographic magnitude as shown on the resulting negative is about the same as the visual, viz., eleventh. On the following night viewed through the 36-inch telescope the spectrum was found to contain bright lines and bands. Last night Professor Campbell measured the positions of some four or five of these bright lines.

As the star is now very faint I have made the accompanying diagram (from a photograph taken Aug. 26) of the region, with the aid of which it will be easy to identify the star. The brightest star in the diagram is 8.7 magnitude; the faintest, about 11.5 magnitude.

J. M. SCHAEBERLE.

Nova Aurigæ a Nebula.—Mr. Campbell having announced that Nova Aurigæ was again bright, I have taken the first opportunity to examine it with the 36-inch, and this morning found the object to be really a small bright nebula with a 10th magnitude nucleus. The nebula is of that class which appear stellar with low powers or insufficient optical means. With the micrometer the nebulosity was found to be 3″ in diameter—a fainter nebulosity still surrounded this and was perhaps ½′ in diameter. The position of the Nova was carefully measured with reference to two small stars previously used by Mr. Burnham in his careful chart of the region about the Nova published in M. N., Vol. LII., No. 6.

Following are his measures and my own present ones:

	A and E.		
1892.14	323°.6	74″.24	β 3 *n*
1892.64	323 .3	74 .24	B 1 *n*
	A and F.		
1892.12	32°.4	85″.05	β 4 *n*
1892.64	32 .6	85 .03	B 1 *n*

These measures show conclusively that the Nova has not sensibly changed its position in six months. The nucleus is one-tenth or two-tenths magnitude less than the star F, though the nebula as a whole is brighter.

I am familiar with other nebulæ that are exactly similar to this object.

Mt. Hamilton, 1892, Aug. 20. E. E. BARNARD.

A New Variable Star.—At the request of Professor Holden, Director of the Lick Observatory, the discovery by Professor Schaeberle, of that Observatory, of a new variable star is announced in the present article. The discovery was made during the examination for another purpose of a series of photographs made at Harvard College Observatory with the eight-inch photographic telescope of that institution. Upon one of these plates, taken December 18, 1891, Professor Schaeberle noticed a star of the magnitude 9.5 which could not be certainly found on another plate taken January 24, 1891, and may consequently be assumed to have been at that time much fainter than the eleventh magnitude. Recent visual observations, made by Professor Schaeberle with a six-inch telescope, showed a star of about the eleventh magnitude in the place of the suspected variable, and these observations were confirmed by a photograph which he took with the Willard lens and an exposure of 60^m. On August 27, 1892, the spectrum of the star was examined with the 36-inch telescope of the Lick Observatory and found to contain bright lines.

From additional photographs of the same region, preserved at Harvard College Observatory, the following photographic magnitudes of the star have been obtained:

1890	Oct.	31	9.6	1891	Nov.	25	10.0
	Dec.	20	10.2		Dec.	16	10.4
	Dec.	29	11.0		Dec.	17	10.3
1891	March	14	<11.7	1892	Jan.	5	10.9
	Nov.	25	10.1				

These photographs, accordingly, confirm the fact of the variability of the star. Its position for 1900 is as follows: R. A. $3^h\ 5^m.5$; Decl. $+ 14° 24'$.

EDWARD C. PICKERING,
Director of Harvard College Observatory.

Cambridge, U. S., Sept. 9, 1892.

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